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PROCEEDINGS

OF

THE AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.

TWENTY-THIRD MEETING,

HELD AT

HARTFORD, CONN.

AUGUST, 1874.

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F. W. PUTNAM,

Permanent Secretary.

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OFFICERS

OF

THE HARTFORD MEETING.

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ASA GRAY, of Cambridge,
E. S. MORSE, of Salem,
B. A. GOULD, of Boston,
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*Not present. †Until Tuesday.
‡Elected on Tuesday to fill vacancy caused by absence of Prof. Rogers.

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Rev. Prof. W. N. Rice,
Prof. J. M. Van Vleck.

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B. A. GOULD, of Cambridge,
JOSEPH HENRY, of Washington,
J. E. HILGARD, of Washington,
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BENJAMIN PEIRCE, of Cambridge,
W. B. ROGERS, of Boston,
J. LAWRENCE SMITH, Louisville.
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2. *Committee to Memorialize the Legislature of New York for a New Survey of Niagara Falls.*

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JAMES HALL, of Albany,
WILLIAM E. LOGAN, of Montreal,
G. W. HOLLEY, of Niagara Falls.

3. *Committee to Report on the Best Methods of Organizing and Conducting State Geological Surveys.*

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JAMES HALL, of Albany,
J. S. NEWBERRY, of Cleveland,
ALEXANDER WINCHELL, Syracuse,
T. STERRY HUNT, of Boston,
BENJAMIN PEIRCE, of Cambridge.

4. *Committee to Memorialize Congress in relation to a Geological Map of the United States.*

This committee consists of such of the State Geologists as will join in the memorial.

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C. H. HITCHCOCK, of Hanover, *Secretary.*

5. *Committee on the Elizabeth Thompson Donation.*

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J. L. LeConte, of Philadelphia,
P. H. Van der Weyde, of N. Y.,
Thomas Hill, of Portland,
James Hall, of Albany,
T. Sterry Hunt, of Boston,
F. W. Putnam, of Salem.

6. *Committee to Report on the Principles of Nomenclature.*

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James Hall, of Albany,
J. S. Newberry, of Cleveland,
Alexander Agassiz, Cambridge,
Theodore Gill, of Washington.

7. *Committee to Memorialize Congress and State Legislatures regarding the Cultivation of Timber, and the Preservation of Forests.*

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Asa Gray, of Cambridge,
G. B. Emerson, of Boston,
J. D. Whitney, of San Francisco,
J. S. Newberry, of Cleveland,
L. H. Morgan, of Rochester,
Chas. Whittlesey, of Cleveland,
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1. *Committee to act with the Standing Committee in Nomination of Officers for the Meeting of* 1875.

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E. B. Elliott, of Washington,
S. W. Johnson, of New Haven,
S. D. Tillman, of New York.

SECTION B.

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Henry Wheatland, of Salem,
W. C. Kerr, of Raleigh,
S. S. Haldemann, of Chickis.

2. *Committee to Audit the Accounts of the Permanent Secretary and Treasurer.*

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TREASURER.

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Officers of the Preceding Meeting.—John L. LeConte, of Philadelphia; C. S. Lyman, of New Haven; A. C. Hamlin, of Bangor.

General and Sectional Officers of the Detroit Meeting.—As given above.

From the Association at large.—Six Fellows to be elected on the first day of the meeting.

LOCAL COMMITTEE

FOR

THE DETROIT MEETING.

Chairman:—His Excellency, Governor JOHN J. BAGLEY.

Treasurer:—WILLIAM A. BUTLER, Esq.

Secretary:—FREDERICK WOOLFENDEN, Esq.

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Supervision:—J. J. Bagley, Hugh Moffatt, E. A. Brush, T. H. Hinchman, James V. Campbell, W. A. Butler.

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Entertainment:—W. J. Chittenden, C. S. Witbeck, Edward Lyon, J. M. Maxwell, Hiram Walker, E. C. Walker, J. E. King, D. M. Dickinson, Jerome Croul.

Over one hundred other citizens as members of the Committee at large.

MEETINGS AND OFFICERS OF THE AMERICAN ASSOCIATION OF GEOLOGISTS AND NATURALISTS.

MEETING.	DATE.	PLACE.	CHAIRMAN.	SECRETARY.	ASSIST. SEC'Y.	TREASURER.
1st	April 2, 1840,	Philadelphia,	Edward Hitchcock,*	L. C. Beck,*		
2d	April 5, 1841,	Philadelphia,	Benjamin Silliman,*	L. C. Beck,*	{ B. Silliman, Jr., C. B. Trego.	
3d	April 25, 1842,	Boston,	S. G. Morton,*	C. T. Jackson,	{ J. D. Whitney, M. B. Williams,	
4th	April 26, 1843,	Albany,	Henry D. Rogers,*	B. Silliman, Jr.,		John Locke.*
5th	May 8, 1844,	Washington,	John Locke,*	{ B. Silliman, Jr., O. P. Hubbard.		Douglas Houghton.*
6th	April 30, 1845,	New Haven,	Wm. B. Rogers,	{ B. Silliman, Jr., J. Lawrence Smith,		Douglas Houghton.*
7th	Sept. 2, 1846,	New York,	C. T. Jackson,	B. Silliman, jr.,		E. C. Herrick.*
8th	Sept. 20, 1847,	Boston,	Wm. B. Rogers,†	Jeffries Wyman.*		B. Silliman, jr.

* Deceased.

†Professor ROGERS, as chairman of this last meeting, called the first meeting of the new Association to order and presided until it was fully organized by the adoption of a constitution. As he was thus the first presiding officer of the new Association, it was directed at the Hartford meeting that his name be placed at the head of the Past Presidents of the American Association for the Advancement of Science.

MEETINGS AND OFFICERS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

MEETING.	DATE.	PLACE.	PRESIDENT.	VICE-PRESIDENT.	GENERAL SECRETARY.	PERMANENT SEC'Y.	TREASURER.
1st	Sept. 20, 1848,	Philadelphia, Pa.,	W. C. Redfield,*		Walter R. Johnson,*		Jeffries Wyman.*
2d	Aug. 14, 1849,	Cambridge, Mass.,	Joseph Henry,		E. N. Horsford, 1		A. L. Elwyn.
3d	Mar. 12, 1850,	Charleston, S. C.,	A. D. Bache,* 2		L. R. Gibbs, 3		St. J. Ravenel.* 4
4th	Aug. 19, 1850,	New Haven, Conn.,	A. D. Bache,*		E. C. Herrick,*		A. L. Elwyn.
5th	May 5, 1851,	Cincinnati, Ohio,	A. D. Bache,*		W. B. Rogers, 5	S. F. Baird,	S. F. Baird. 6
6th	Aug. 19, 1851,	Albany, N. Y.,	Louis Agassiz,*		W. B. Rogers,	S. F. Baird,	A. L. Elwyn.
7th	July 28, 1853,	Cleveland, Ohio,	Benjamin Pierce,		S. St. John, 7	S. F. Baird,	A. L. Elwyn.
8th	April 26, 1854,	Washington, D.C.,	J. D. Dana,		J. Lawrence Smith,	Joseph Lovering,	J. L. LeConte. 8
9th	Aug. 15, 1855,	Providence, R. I.,	John Torrey,*		Wolcott Gibbs,	Joseph Lovering,	A. L. Elwyn.
10th	Aug. 20, 1856,	Albany, N. Y.,	James Hall,		B. A. Gould,	Joseph Lovering,	A. L. Elwyn.
11th	Aug. 12, 1857,	Montreal, Canada.	Alexis Caswell, 9	Alexis Caswell,	John LeConte,	Joseph Lovering,	A. L. Elwyn.
12th	April 28, 1858,	Baltimore, Md.,	Alexis Caswell, 10	John E. Holbrook,*†	W. M. Gillespie,* 11	Joseph Lovering,	A. L. Elwyn.
13th	Aug. 3, 1859,	Springfield, Mass.,	Stephen Alexander,	Edward Hitchcock,*	William Chauvenet,*	Joseph Lovering,	A. L. Elwyn.
14th	Aug. 1, 1860,	Newport, R. I.,	Isaac Lea,	B. A. Gould,	Joseph LeConte,	Joseph Lovering,	A. L. Elwyn.
15th	Aug. 15, 1866.	Buffalo, N. Y.,	F. A. P. Barnard,	A. A. Gould,* 12	Elias Loomis, 13	Joseph Lovering,	A. L. Elwyn.†
16th	Aug. 21, 1867,	Burlington, Vt.,	J. S. Newberry,	Wolcott Gibbs,	C. S. Lyman,	Joseph Lovering,	A. L. Elwyn.†
17th	Aug. 5, 1868,	Chicago, Ill.,	B. A. Gould,	Charles Whittlesey,	Simon Newcomb, 14	Joseph Lovering,	A. L. Elwyn.†
18th	Aug. 18, 1869,	Salem, Mass.,	J. W. Foster,*	O. N. Rood,	O. C. Marsh,	F. W. Putnam, 15	A. L. Elwyn.†
19th	Aug. 17, 1870,	Troy, N. Y.,	T. S. Hunt, 16	T. S. Hunt,	F. W. Putnam, 17	Joseph Lovering,	A. L. Elwyn.†
20th	Aug. 16, 1871,	Indianapolis, Ind.,	Asa Gray,	G. F. Barker,	F. W. Putnam,	Joseph Lovering,	W. S. Vaux.
21st	Aug. 15, 1872,	Dubuque, Iowa,	J. Lawrence Smith,	Alex. Winchell,	E. S. Morse,	Joseph Lovering,	W. S. Vaux.
22d	Aug. 20, 1873,	Portland, Me.,	Joseph Lovering,	A. H. Worthen,†	C. A. White,	F. W. Putnam,	W. S. Vaux.
23d	Aug. 12, 1874,	Hartford, Conn.,	J. L. LeConte,	C. S. Lyman,	A. C. Hamlin,	F. W. Putnam,	W. S. Vaux.†

1. In place of Jeffries Wyman, *not present.*
2. In place of Joseph Henry, *not present.*
3. In place of E. C. Herrick, *not present.*
4. In place of A. L. Elwyn, *not present.*
5. In place of E. C. Herrick, *not present.*
6. In place of A. L. Elwyn, *not present.*
7. In place of J. D. Dana, *not present.*
8. In place of A. L. Elwyn, *not present.*
9. In place of J. W. Bailey, *deceased.*
10. In place of Jeffries Wyman, *not present.*
11. In place of Wm. Chauvenet, *not present.*
12. In place of R. W. Gibbes, *not present.*
13. In place of W. P. Trowbridge, *not present.*
14. In place of A. P. Rockwell, *called home* 1st *day.*
15. In place of Joseph Lovering, *in Europe.*
16. In place of Wm. Chauvenet, *too ill to be present.*
17. In place of C. F. Hartt, *in Brazil.*

* Deceased † Not present at the meeting.

COMMONWEALTH OF MASSACHUSETTS.

IN THE YEAR ONE THOUSAND EIGHT HUNDRED AND SEVENTY-FOUR.

AN ACT

To Incorporate the "American Association for the Advancement of Science."

Be it enacted by the Senate and House of Representatives, in General Court assembled, and by the authority of the same, as follows:

Section 1. Joseph Henry of Washington, Benjamin Pierce of Cambridge, James D. Dana of New Haven, James Hall of Albany, Alexis Caswell of Providence, Stephen Alexander of Princeton, Isaac Lea of Philadelphia, F. A. P. Barnard of New York, John S. Newberry of Cleveland, B. A. Gould of Cambridge, T. Sterry Hunt of Boston, Asa Gray of Cambridge, J. Lawrence Smith of Louisville, Joseph Lovering of Cambridge and John LeConte of Philadelphia, their associates, the officers and members of the Association, known as the "American Association for the Advancement of Science," and their successors, are hereby made a corporation by the name of the "American Association for the Advancement of Science," for the purpose of receiving, purchasing, holding and conveying real and personal property, which it now is, or hereafter may be possessed of, with all the powers and privileges, and subject to the restrictions, duties and liabilities set forth in the general laws which now or hereafter may be in force and applicable to such corporations.

Section 2. Said corporation may have and hold by purchase, grant, gift or otherwise, real estate not exceeding one hundred thousand dollars in value, and personal estate of the value of two hundred and fifty thousand dollars.

Section 3. Any two of the corporators above named are hereby authorized to call the first meeting of the said corporation in the month of August next ensuing, by notice thereof "by mail," to each member of the said Association.

Section 4. This act shall take effect upon its passage.

House of Representatives, March 10, 1874.
Passed to be enacted,
John E. Sanford, *Speaker.*

In Senate, March 17, 1874.
Passed to be enacted,
Geo. B. Loring, *President.*

March 19, 1874.
Approved,
W. B. Washburn.

Secretary's Department,
Boston, April 3, 1874.
A true copy, Attest:
David Pulsifer,
Deputy Secretary of the Commonwealth.

CONSTITUTION

OF THE

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

Incorporated by Act of the General Court of the Commonwealth of Massachusetts.

Objects.

Article 1. The objects of the Association are, by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of America, to give a stronger and more general impulse and more systematic direction to scientific research, and to procure for the labors of scientific men increased facilities and a wider usefulness.

Members, Fellows, Patrons and Honorary Fellows.

Art. 2. The Association shall consist of Members, Fellows, Patrons and Honorary Fellows.

Art. 3. Any person may become a Member of the Association upon recommendation in writing by two members or fellows, nomination by the Standing Committee, and election by a majority of the members and fellows present in general session.

Art. 4. Fellows shall be nominated by the Standing Committee from such of the members as are professionally engaged in science, or have by their labors aided in advancing science. The election of fellows shall be by ballot and a majority vote of the members and fellows present in general session. But all persons who may be members at the time of the adoption of this constitution may become fellows by signifying their desire to this effect before the first day of August, 1875.

Art. 5. Any person paying to the Association the sum of one thousand dollars shall be classed as a Patron, and shall be entitled to all the privileges of a member and to all its publications.

ART. 6. Honorary Fellows of the Association, to the number of ten for each section, may be elected; the nominations to be made by the Standing Committee and approved by ballot in the respective sections before election by ballot in general session. Honorary Fellows shall be entitled to all the privileges of fellows and shall be exempt from all fees and assessments, and entitled to all publications of the Association issued after the date of their election.

ART. 7. The name of any member or fellow two years in arrears for annual dues shall be erased from the list of the Association, provided that two notices of indebtedness, at an interval of at least three months, shall have been given; and no such person shall be restored until he has paid his arrearages or has been reëlected.

ART. 8. No member or fellow shall take part in the organization or business of both sections at the same meeting.

OFFICERS.

ART. 9. The Officers of the Association shall be elected by ballot in general session from the fellows and shall consist of a President, two Vice Presidents, a General Secretary, a Permanent Secretary, a Treasurer, a Secretary of Section A, and a Secretary of Section B; these, with the exception of the Permanent Secretary, shall be elected at each meeting for the following one, and, with the exception of the Treasurer and the Permanent Secretary, shall not be reëligible for the next two meetings. The Permanent Secretary shall be elected at each fifth meeting.

ART. 10. The President, or, in his absence, one of the Vice Presidents, shall preside at all general sessions of the Association and at all meetings of the Standing Committee. It shall also be the duty of the President to give an address at a general session of the Association at the meeting following that over which he presided.

ART. 11. The Vice Presidents shall be the presiding officers of Sections A and B, and of the Sectional Commiittees, and it shall be part of their duty to give an address, each before his respective section, at such time as the section shall determine. The Vice Presidents may request their respective sections to appoint temporary chairmen to preside over the sessions of the sections, but shall not delegate their other duties.

ART. 12. The General Secretary shall be the Secretary of all general sessions of the Association, and of all sessions of the Standing Committee, and shall keep a record of the business of these sessions. He

shall receive the records from the Secretaries of the Sections, which, after examination, he shall transmit with his own records to the Permanent Secretary within two weeks after the adjournment of the meeting. He shall receive proposals for membership and bring them before the Standing Committee. He shall give to the Secretary of each Section the list of papers assigned to it by the Standing Committee.

ART. 13. The Permanent Secretary shall be the executive officer of the Association under the direction of the Standing Committee. He shall attend to all business not specially referred to committees nor otherwise constitutionally provided for. He shall keep an account of all business that he has transacted for the Association, and make annually at the first meeting of the Standing Committee, a report which shall be laid before the Association. He shall attend to the printing and distribution of the annual volume of Proceedings, and all other printing ordered by the Association. He shall issue a circular of information to members and fellows at least four months before each meeting, and shall, in connection with the Local Committee, make all necessary arrangements for the meetings of the Association. He shall provide the Secretaries of the Association with such books and stationery as may be required for their records and business, and shall provide members and fellows with such blank forms as may be required for facilitating the business of the Association. He shall collect all assessments and admission fees, and notify members and fellows of their election, and of any arrearages. He shall receive, and bring before the Standing Committee, the titles and abstracts of papers proposed to be read before the Association. He shall keep an account of all receipts and expenditures of the Association, and report the same annually at the first meeting of the Standing Committee, and, at the close of each year, shall pay over to the Treasurer such unexpended funds as the Standing Committee may direct. He shall receive and hold in trust for the Association all books, pamphlets and manuscripts belonging to the Association, and allow the use of the same under the provisions of the Constitution and the orders of the Standing Committee. He shall receive all communications addressed to the Association during the interval between meetings, and properly attend to the same. He shall at each meeting report the names of fellows and members who have died since the preceding meeting. He shall be allowed a salary which shall be determined by the Standing Committee, and may employ a clerk at such compensation as may be agreed upon by the Standing Committee.

ART. 14. The Treasurer shall invest the funds received by him in such securities as may be directed by the Standing Committee. He shall annually present to the Standing Committee an account of the funds in his charge. No expenditure of the principal in the hands of the Treasurer shall be made without a unanimous vote of the Standing Committee, and no expenditure of the income received by the Treasurer shall be made without a two-thirds vote of the Standing Committee.

ART. 15. The Secretaries of Sections A and B shall keep the records of their respective sections, and, at the close of the meeting, give the same, including the records of subsections, to the General Secretary. They shall also be the secretaries of the sectional committees.

ART. 16. In case of a vacancy in the office of the President, one of the Vice Presidents shall be elected by the Standing Committee as the President of the meeting. Vacancies in the offices of Vice President, General Secretary, Permanent Secretary and Treasurer, shall be filled by nomination of the Standing Committee and election by ballot in general session. A vacancy in the office of Secretary of a Section shall be filled by nomination and election by ballot in the section.

ART. 17. The Standing Committee shall consist of the past Presidents, the President, the Vice Presidents, the four Secretaries, the Treasurer, with the above named officers of the preceding meeting, and six fellows elected by ballot after open nomination at the first general session. The members present at any regularly called meeting of the Committee, provided there are at least five, shall form a quorum for the transaction of business. The Standing Committee shall meet on the day preceding each annual meeting of the Association, and arrange the programme for the first day of the sessions. The time and place of this first meeting shall be designated by the Permanent Secretary. Unless otherwise agreed upon, regular meetings of the Committee shall be held in the committee room at 9 o'clock, A.M., on each day of the meeting of the Association. Special meetings of the Committee may be called at any time by the President. The Standing Committee shall be the board of supervision of the Association, and no business shall be transacted by the Association that has not first been referred to, or originated with, the Committee. The special business of the Committee shall be: to receive and assign papers to the respective sections; to examine and, if necessary, to exclude papers; to decide which papers, discussions and other proceedings shall be published, and to have the general direction of

the publications of the Association; to manage the financial affairs of the Association; to arrange the business and programmes for general sessions; to appoint general sessions for the evening; to suggest subjects for discussion, investigation or reports; to nominate members and fellows; to receive and act upon all invitations extended to the Association and report the same at a general session of the Association.

ART. 18. The Nominating Committee shall consist of the Standing Committee, and four members or fellows elected by each of the sections. It shall be the duty of this Committee to meet at the call of the President and nominate the general officers for the following meeting of the Association. It shall also be the duty of this Committee to recommend the time and place for the next meeting. The Vice Presidents and Secretaries of the Sections shall be recommended to the Nominating Committee by sub-committees consisting of the Vice Presidents and Secretaries, and the four persons elected by each section under the first clause of this article.

MEETINGS.

ART. 19. The Association shall hold public meetings annually, for one week or longer, at such time and place as may be determined by vote of the Association, and the preliminary arrangements for each meeting shall be made by the Local Committee, in conjunction with the Permanent Secretary and such other persons as the Standing Committee may designate.

ART. 20. General Sessions shall be held at 10 o'clock, A. M., unless otherwise ordered, on every day of the meeting, Sunday excepted, and at such other times as may be appointed by the Standing Committee.

SECTIONS AND SUBSECTIONS.

ART. 21. The Association shall be divided into two Sections, namely: **A** (*Mathematics*, *Astronomy*, *Physics*, *Chemistry* and *Mineralogy*) and **B** (*Geology*, *Zoology*, *Botany* and *Anthropology*). Either Section may, at its pleasure, form temporary or permanent subsections for the reading of papers.

ART. 22. Immediately on the organization of a Section there shall be three fellows elected by ballot after open nomination, who, with the Vice President and Secretary, and the Chairman and Secretary of the subsections, shall form its Sectional Committee. The Sectional Com-

mittees shall have power to fill vacancies in their own numbers. There shall be no sectional meeting during a general session.

ART. 23. When any Subsection organizes, it shall elect a Chairman and Secretary and report the result to the Secretary of its Section. The Secretary of a Subsection shall, at the close of the meeting, transmit his records to the Secretary of the Section. Any Permanent Subsection may elect its Chairman for the ensuing meeting.

ART. 24. No paper shall be read in any Section or Subsection until it has been placed on the programme of the day by the Sectional Committees.

SECTIONAL COMMITTEES.

ART. 25. The Sectional Committees shall arrange and direct the business of their respective sections. They shall prepare the daily programmes and give them to the Permanent Secretary for printing at the earliest moment practicable. No titles of papers shall be entered on the daily programmes except such as have passed the Standing Committee. No change shall be made in the programme for the day without the consent of the Sectional Committee. The Sectional Committees may refuse to place the title of any paper on the programme; but every such title, with the abstract of the paper or the paper itself must be returned to the Standing Committee with the reasons why it was refused.

ART. 26. The Sectional Committees shall examine all papers and abstracts referred to the sections, and they shall not place on the programme any paper inconsistent with the character of the Association; and to this end they have power to call for any paper, the character of which may not be sufficiently understood from the abstract submitted.

PAPERS AND COMMUNICATIONS.

ART. 27. All members and fellows must forward to the Permanent Secretary, as early as possible, and when practicable before the convening of the Association, full titles of all the papers which they propose to present during the meeting, with a statement of the time that each will occupy in delivery, and also such abstracts of their contents as will give a general idea of their nature; and no title shall be referred by the Standing Committee to the Sectional Committee until an abstract of the paper or the paper itself has been received.

ART. 28. If the author of any paper be not ready at the time assigned, the title may be dropped to the bottom of the list.

Reports of Meetings.

Art. 29. Whenever practicable, the proceedings and discussions at general sessions, sections and subsections shall be reported by professional reporters, but such reports shall not appear in print as the official reports of the Association unless revised by the secretaries.

Printed Proceedings.

Art. 30. The Permanent Secretary shall have the Proceedings of each meeting printed in an octavo volume as soon after the meeting as possible, beginning one month after adjournment. Authors must prepare their papers ready for the press and forward them to the Permanent Secretary within this interval, otherwise only the abstracts or titles will appear in the printed volumes. The Standing Committee shall have power to print an abstract only of any paper. Whenever practicable, proofs shall be forwarded to authors for revision. If any additions or substantial alterations are made by the author of a paper after its submission to the Secretary, the same shall be distinctly indicated. Illustrations must be provided for by the authors of the papers, or by a special appropriation from the Standing Committee. Immediately on publication of the volume, a copy shall be forwarded to every member and fellow of the Association who shall have paid the assessment for the meeting to which it relates, and it shall also be offered for sale by the Permanent Secretary at such price as may be determined by the Standing Committee. The Standing Committee shall also designate the institutions to which copies shall be distributed.

Local Committee.

Art. 31. The Local Committee shall consist of persons interested in the objects of the Association and residing at or near the place of the proposed meeting. It is expected that the Local Committee, assisted by the officers of the Association, will make all essential arrangements for the meeting, and issue a circular giving necessary particulars, at least one month before the meeting.

Library of the Association.

Art. 32. All books and pamphlets received by the Association shall be in the charge of the Permanent Secretary, who shall have a list of the same printed and shall furnish a copy to any member or fellow on application. Members and fellows who have paid their assessments in full

shall be allowed to call for books and pamphlets, which shall be delivered to them at their expense, on their giving a receipt agreeing to make good any loss or damage and to return the same free of expense to the Secretary at the time specified in the receipt given. All books and pamphlets in circulation must be returned at each meeting, or the value of any not so returned paid to the Permanent Secretary. Not more than ten books, including volumes, parts of volumes, and pamphlets, shall be held at one time by any member or fellow. Any book may be withheld from circulation by order of the Standing Committee.

Admission Fee and Assessments.

Art. 33. The admission fee for members shall be five dollars in addition to the annual assessment. On the election of any member as a fellow an additional fee of two dollars shall be paid.

Art. 34. The annual assessment for members and fellows shall be three dollars.

Art. 35. Any member or fellow who shall pay the sum of fifty dollars to the Association, at any one time, shall be exempt from all further assessments, but this payment shall not entitle him to the publications of the Association, and all money thus received shall be invested as a permanent fund, the income of which shall be used only to assist in original research.

Art. 36. All admission fees and assessments must be paid to the Permanent Secretary, who shall give proper receipts for the same.

Accounts.

Art. 37. The accounts of the Permanent Secretary and of the Treasurer shall be audited annually, by two auditors appointed by the Standing Committee.

Alterations of the Constitution.

Art. 38. No part of this Constitution shall be amended or annulled, without the concurrence of three-fourths of the members and fellows present in general session, after notice given at a general session of a preceding meeting of the Association.

ORDER OF PROCEEDINGS

IN

ORGANIZING A MEETING.

1. The retiring President introduces the President elect, who takes the chair.
2. Formalities of welcome of the Association as may be arranged by the Local Committee.
3. Report of the list of papers on the register and their reference to the Sections.
4. Other reports.
5. Announcements of arrangements by the Local Committee.
6. Elections to complete the Standing Committee.
7. Election of members.
8. Election of fellows.
9. Unenumerated business.
10. Adjournment to meet in Sections.

This order, so far as applicable, to be followed in subsequent General Sessions.

MEMBERS

OF THE

AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.[1]

PATRON.[2]

Thompson, Mrs. Elizabeth, 46 East Tenth St., New York (22).

LIFE MEMBERS.[3]

Case, Leonard, Cleveland, Ohio (15).
Robertson, Thomas D., Rockford, Ill. (10).
Warner, James D., 199 Baltic St., Brooklyn, N. Y. (18).

MEMBERS.

Abbe, Prof. Cleveland, Washington, D. C. (16).
Abbe, George W., 32 East 20th St., New York City (23).
Abbot, Miss Elizabeth O., No. 10 Thomas St., Providence, R. I. (20).
Adcock, Prof. Robert J., Monmouth, Warren Co., Ill. (21).
Agassiz, Alexander, Curator Mus. Comp. Zoology, Cambridge, Mass. (18).
Aikin, Prof. W. E. A., Baltimore, Md. (12).
Ainsworth, Frank B., Supt. Ind. House of Refuge, Plainfield, Ind. (20).
Albert, Augustus J., Baltimore, Md. (12).
Alexander, John S., 1935 Arch St., Philadelphia, Penn. (20).

[1] The numbers in parentheses indicate the meeting at which the member was elected. The Constitution requires that the names of all members two or more years in arrears shall be omitted from the list, but their names will be restored on payment of arrearages. Members not in arrears are entitled to the annual volume of Proceedings.

[2] Persons contributing one thousand dollars or more to the Association are classed as Patrons, are exempt from the annual assessments and are entitled to the annual volume.

[3] Any Member or Fellow may become a Life Member by the payment of fifty dollars. The money derived from Life Memberships is invested as a fund, the income of which is to be used only to aid in original research. Life members are exempt from the annual assessment, but are not entitled to the annual volume.

Allen, Bennet R., Insurance Agent, Hartford, Conn. (23).
Allen, Joel A., Mus. Comp. Zool., Cambridge, Mass. (18).
Allen, J. M., Hartford, Conn. (22).
Allen, Stephen M., 28 State St., Boston, Mass. (23).
Allen, Zachariah, Providence, R. I. (1).
Allyn, Mrs. Clarence, Gale's Ferry, New London Co., Conn. (22).
Andrews, Prof. E. B., Lancaster, Ohio (7).
Andrews, Dr. Edmund, 6 Sixteenth St., Chicago, Ill. (22).
Appleton, Prof. John H., Brown University, Providence, R. I. (22).
Armsby, Henry P., Box 1099, Fitchburg, Mass. (23).
Arthur, J. C., Charles City, Iowa (21).
Atwater, Samuel T., 166 Washington St., Chicago, Ill. (17).
Atwater, Mrs. Samuel T., 166 Washington St., Chicago, Ill. (17).
Austin, E. P., Box, 434 Cambridge, Mass. (18).
Avery, Alida C., Denver, Col. (20).

Babbitt, Henry S., M. D., Columbus, Ohio (23).
Babcock, George, Sup't Rensselaer Iron Works, Troy, N. Y. (19).
Bailey, Prof. Loring W., University of Frederickton, N. B. (18).
Baird, Lyman, 90 La Salle St., Chicago, Ill. (17).
Baird, Prof. S. F., Smithsonian Institution, Washington, D. C. (1).
Baker, Prof. T. R., Millersville, Lancaster Co., Penn. (22).
Balch, David M., Salem, Mass. (22).
Barker, Prof. G. F., University of Penn., Philadelphia, Pa. (13).
Barnard, Jas. M., Boston, Mass. (18).
Barnard, Gen. J. G., U.S.A., Army Building, New York (14).
Bartlett, Frank L., Hanover, Me. (22).
Basnett, Thomas, St. Nicholas, Fla. (8).
Bassett, George W., M. D., Vandalia, Ill. (20).
Bassett, H. F., Waterbury, Conn. (23).
Batchelder, John M., No. 18 Pemberton Sq., Boston, Mass. (8).
Batterson, J. G., Hartford, Conn. (23).
Beach, Charles M., Merchant, Hartford, Conn. (23).
Beach, J. Watson, Merchant, Hartford, Conn. (23).
Beach, W. H., Dubuque, Iowa (21).
Becker, Dr. Alexander R., 265 Benefit St., Providence, R. I. (22).
Bell, James D., Office of Daily Graphic, New York (20).
Bell, John J., Exeter, N. H. (22).
Benjamin, E. B., 10 Barclay St., New York (19).
Bessey, Prof. C. E., Agricultural College, Ames, Iowa (21).
Bethune, Rev. Chas. J. S., Trinity Coll. School, Port Hope, Canada (18).
Bickmore, Prof. Albert S., Arsenal Building, 6 Central Park, N. Y. (17).
Blake, Frank, Jr., Ass't U. S. C. Survey, Newton Lower Falls, Mass. (23).
Blake, Dr. Mary J. Safford, 4 Boylston Place, Boston, Mass. (21).
Blake, W. P., Geologist, New Haven, Conn. (2).
Blakeslee, B. F., Stock Broker, Hartford, Conn. (23).
Blatchford, Elphalet W., Chicago, Ill. (17).

Bliss, Porter C., Journalist, 17 Beacon St., Boston, Mass. (18).
Boadle, John, Haddonfield, N. J. (20).
Bolles, Rev. E. C., Salem, Mass. (17).
Bolton, Dr. H. C., School of Mines, Columbia College, New York (17).
Bontecou, Dr. R. B., Troy, N. Y. (19).
Bouvé, Thomas T., Pres't Boston Soc. Nat. History, Boston, Mass. (1).
Bowditch, Dr. Henry I., 113 Boylston St., Boston, Mass. (2).
Bowen, Silas T., Indianapolis, Ind. (20).
Boynton, Miss Susan P., Box 150, Lynn, Mass. (19).
Brackett, Prof. C. F., College of New Jersey, Princeton, N. J. (19).
Bradley, L., 9 Exchange Place, Jersey City, N. J. (15).
Breneman, A. A., Lancaster, Penn. (20).
Brevoort, J. Carson, Brooklyn, N. Y. (1).
Brewer, Prof. W. H., New Haven, Conn. (20).
Briggs, Albert D., 7 Fort Block, Springfield, Mass. (13).
Briggs, S. A., Box 545, Chicago, Ill. (17).
Brigham, Rev. Charles H., Ann Arbor, Mich. (17).
Bristol, Eugene S., New Haven, Conn. (17).
Brocklesby, Prof. John, Trinity College, Hartford, Conn. (23).
Brooks, William K., Cambridge, Mass. (23).
Broomall, Hon. John M., Media, Del. Co., Pa. (23).
Bryan, Oliver N., Accokeek P. O., Prince George's Co., Md. (18).
Buckhout, W. A., State College, Pa. (20).
Burbank, L. S., Woburn, Mass. (18).
Burgess, Edward, Sec'y Nat. Hist. Society, Boston, Mass. (22).
Burton, H. J., jr., Boston, Mass. (22).
Bush, Stephen, Waterford, N. Y. (19).

Caldwell, Prof. George C., Cornell University, Ithaca, N. Y. (23).
Campbell, Mrs. Mary H., Crawfordsville, Ind. (22).
Canby, William M., 1101 Delaware Ave., Wilmington, Del. (17).
Capen, Miss Bessie T., Girls' High School, Boston, Mass. (23).
Carmichael, Prof. Henry, Bowdoin College, Brunswick, Me. (21).
Carpenter, Prof. G. C., Simpson Centenary College, Indianola, Iowa (22).
Carrington, Col. Henry B., U. S. A., Prof. in Wabash College, Crawfordsville, Ind. (20).
Case, L. B., Richmond, Indiana (17).
Caswell, Prof. Alexis, Providence, R. I. (2).
Cattell, William C., President Lafayette College, Easton, Penn. (15).
Chadbourne, Prof. P. A., Pres't Williams Coll., Williamstown, Mass. (10).
Chadeayne, Miss E., 35 Grand St., Jersey City, N. J. (22).
Chamberlin, T. C., Beloit College, Beloit, Wis. (21).
Chandler, C. F., School of Mines, Columbia Coll., New York (19).
Chanute, O., Chief Engineer Erie Railway Co., New York (17).
Chapman, F. M., 90 La Salle St., Chicago, Ill. (17).
Chase, Prof. Pliny E., Haverford College, Haverford, Penn. (18).
Chase, R. Stuart, 16 Merrimack St., Haverhill, Mass. (18).

Chesbrough, E. S., Chicago, Ill. (2).
Chickering, Prof. J. W., jr., Deaf Mute College, Washington, D. C. (22).
Clark, John E., 45 Clark St., New Haven, Conn. (17).
Coffinberry, W. L., Grand Rapids, Mich. (20).
Cogswell, Dr. George, Bradford, Mass. (18).
Colton, G. Woolworth, 172 William St., New York City (22).
Comstock, Prof. M. L., Galesburg, Ill. (21).
Connerly, Prof. D. C. B., Principal Free School, Birmingham, Ala. (23).
Conser, Prof. E. P., Sand Spring, Iowa (21).
Cook, Prof. George H., Lock Box 5, New Brunswick, N. J. (18).
Cooke, Caleb, Peabody Academy of Science, Salem, Mass. (18).
Cooley, Prof. Le Roy C., N. Y. State Normal School, Albany, N. Y. (19).
Cope, Prof. Edward D., Haddonfield, N. J. (17).
Copes, Dr. Joseph S., care Copes & Ogden, New Orleans, La. (11).
Cornwall, Prof. Henry B., College of New Jersey, Princeton, N. J. (22).
Coxe, Eckley B., Drifton, Jeddo, P. O., Luzerne Co., Pa. (23).
Crawford, Dr. John S., Galena, Ill. (21).
Crocker, Charles F., Lawrence, Mass. (22).
Crocker, Mrs. Charles F., Lawrence, Mass. (21).
Crosby, M. S., Pres't Waterbury Scientific Soc., Waterbury, Conn. (23).
Cummings, John, Woburn, Mass. (18).
Cummings, Rev. Dr. Joseph, Pres't Wesleyan Univ., Middletown, Ct. (13).
Curtis, Dr. Josiah, Ebbitt House, Washington, D. C. (18).
Curtis, Rev. Dr. W. S., Rockford, Ill. (21).
Cutter, John D., 1208 Pacific St., Brooklyn, L. I. (23).

Dana, Edward Salisbury, New Haven, Conn. (23).
Dana, Prof. James D., New Haven, Conn. (1).
Davis, George T., Portland, Me. (23).
Davis, James (1).
Dawson, Dr. J. W., Principal McGill College, Montreal, Can. (10).
Dean, George W., Fall River, Mass. (15).
DeCamp, Dr. William H., Grand Rapids, Mich. (21).
Delano, Joseph C., New Bedford, Mass. (5).
Derby, O. A., Ithaca, N. Y. (23).
Devereux, J. H., President C. C. C. & I. R. R., Cleveland, Ohio (18).
Dickinson, Rev. John, Mount Vernon, N. Y. (23).
Dinwiddie, Robert, 113 Water St., New York (1).
Dixwell, Epes S., Cambridge, Mass. (1).
Dodd, C. M., Williamstown, Mass. (19).
Dodge, Charles R., Washington, D. C. (22).
Doggett, Wm. E., Chicago, Ill. (17).
Doggett, Mrs. Wm. E., Chicago, Ill. (17).
Dolbear, A. Emerson, Bethany, West Va. (20).
Doughty, John W., Newburgh, N. Y. (19).
Downer, Henry E., 103 Woodward Ave., Detroit, Mich. (21).
Drowne, Charles, Rensselaer Polytechnic Institute, Troy, N. Y. (6).

Drummond, Josiah H., Portland, Me. (22).
Dudley, Charles B., University of Pa., Philadelphia, Pa. (23).
Dutton, Capt. Clarence E., U. S. Ordnance Dep't, Washington, D. C. (23).
Dyer, Clarence, Lawrence, Mass. (22).
Dyer, Elisha, 26 Westminster St., Providence, R. I. (9).

Eaton, Prof. James H., Beloit College, Beloit, Wis. (17).
Edgar, George M., Pres't Franklin Female College, Franklin, Ky. (20).
Edwards, A. Mead, M. D., 120 Belleville Ave., Newark, N. J. (18).
Elliott, Arthur H., care Smington Bros., 44 South St., Baltimore, Md. (23).
Elwyn, Alfred L., Philadelphia, Penn. (1).
Emerson, Prof. Benjamin K., Amherst, Mass. (19).
Emerson, Prof. Charles F., Dartmouth College, Hanover, N. H. (22).
Emerson, George B., LL.D., 3 Pemberton Sq., Boston, Mass. (1).
Emerton, James H., Boston, Mass. (18).
Endlech, Frederic N., Smithsonian Institution, Washington, D. C. (22).
Engelmann, Dr. George, St. Louis, Mo. (1).
Ennis, Jacob, Principal Scientific Inst., Philadelphia, Penn. (19).
Ernst, Rev. Carl W., P. O. Box 671, Providence, R. I. (23).
Everts, Miss M. M., care Rev. W. W. Everts, D. D., Chicago, Ill. (22).

Faries, R. J., Wauwatosa, Wis. (21).
Farlow, Dr. W. G., Cambridge, Mass. (20).
Farmer, Moses G., Salem, Mass. (9).
Fellowes, R. S., New Haven, Conn. (18).
Fernald, Prof. Charles H., State Agricultural College, Orono, Me. (22).
Fernald, Prof. M. C., State Agricultural College, Orono, Me. (22).
Ferrell, William, Cambridge, Mass. (11).
Feuchtwanger, Dr. Lewis, 180 Fulton St., New York (11).
Ficklin, Prof. Joseph, University of Missouri, Columbia, Mo. (20).
Fisher, Prof. Davenport, from June 1 to Oct. 1, 642 Marshall St., Milwaukee, Wis.; rest of the year, Annapolis, Md. (17).
Fisk, Rev. Dr. Richmond, jr., Grand Rapids, Mich. (19).
Fitch, Edmund H., Sec. of Toledo Soc. of Nat. Sciences, Toledo, O. (23).
Fitch, O. H., Ashtabula, Ohio (7).
Fletcher, Ingram, care Fletcher & Sharpe, Indianapolis, Ind. (20).
Fletcher, Dr. Wm. B., Indianapolis, Ind. (20).
Ford, Silas W., 24 7th St., Troy, N. Y. (19).
Foreman, Henry L., U. S. Signal Service, Washington, D. C. (23).
Forshey, Col. C. G., New Orleans, La. (21).
Foster, Henry, M. D., Clifton, N. Y. (17).
Franklin, Gen. W. B., Vice Pres't Colt's Armory, Hartford, Conn. (23).
Freeman, H. C., Cobden, Union Co., Ill. (17).
French, Dr. Geo. F., 666 Congress St., Portland, Me. (22).
Frothingham, Rev. Frederick, Buffalo, N. Y. (11).

Fuller, Charles B., 39 Preble St., Portland, Me. (22).
Fulton, Prof. Robert B., University of Miss., Oxford, Miss. (21).

Gage, Rev. William Leonard, Hartford, Conn. (23).
Garbett, Wm. A., 8 and 10 Guild Building, Boston Highlands, Mass. (22).
Garman, S. W., Mus. Comp. Zool., Cambridge, Mass. (20).
Garnett, Algernon S., M. D., Hot Springs, Arkansas (23).
Garrett, Ellwood, Wilmington, Newcastle County, Del. (22).
Gaskill, Joshua, Lockport, N. Y. (22).
Gatling, Dr. R. J., Hartford, Conn. (23).
Gennadius, Panajotes, care J. Gennadius, 32 Bury St., St. James's, London, Eng. (23).
Gilbert, Dexter W., Master Mechanic, Keene, N. H. (23).
Gill, Prof. Theodore, Smithsonian Institution, Washington, D. C. (17).
Gilman, Prof. Daniel C., Pres't University of California, Oakland, Cal. (10).
Glazier, Sarah M. (19).
Goessman, Prof. C. A., State Agricultural College, Amherst, Mass. (18).
Gold, Theodore S., West Cornwall, Conn. (4).
Goodale, Prof. G. L., Botanic Gardens, Cambridge, Mass. (18).
Goode, Prof. George Brown, Middletown, Conn. (22).
Goold, W. N., Sec'y Portland Society Natural History, Portland, Me. (22).
Gould, Prof. B. A., care Hon. Josiah Quincy, 4 Park St., Boston, Mass. (2).
Gould, Sylvester C., Manchester, N. H. (22).
Grant, Mrs. Mary J., Brookfield, Conn. (23).
Gray, Prof. Asa, Botanical Gardens, Cambridge, Mass. (1).
Green, Jacob L., Sec. Mut. Life Ins. Co., Hartford, Conn. (23).
Green, Dr. Samuel E., Blairsville, Penn. (22).
Greene, David M., Dep't S. E. & S., Albany, N. Y. (19).
Gregory, Prof. J. J. H., Marblehead, Mass. (18).
Grinnan, A. G., Orange Court House, Va. (7).
Grote, Aug. R., Sec'y Buffalo Soc. Nat. History, Buffalo, N. Y. (22).
Gunning, William D., Waltham, Mass. (22).
Guyot, Prof. Arnold, Princeton, N. J. (1).

Hadley, George, 1355 Main St., Buffalo, N. Y. (6).
Hagen, Dr. Hermann A., Mus. Comp. Zool., Cambridge, Mass. (17).
Hall, George E., Cleveland, Ohio (19).
Hall, Prof. James, Albany, N. Y. (1).
Hall, Hon. Nathan K. (7).
Hamel, Prof. Thomas E., Laval University, Quebec, Canada (18).
Hanaman, C. E., Troy, N. Y. (19).
Hance, Ebenezer, Fallsington P. O., Bucks County, Penn. (7).
Harrington, Prof. Mark W., Ann Arbor, Mich. (22).
Harrison, Dr. B. F. Wallingford, Conn. (11).
Hart, Rev. Prof. Samuel, Trinity College, Hartford, Conn. (22).
Harvey, Charles W., Sup't Public Schools, Greensburg, Ind. (20).
Harvey, Dr. Leon F., Buffalo, N. Y. (22).

Hatfield, Marcus P., M. D., Chicago, Ill. (23).
Hawes, George W., New Haven, Conn. (23).
Hawkins, Dr. B. W., 9 Beacon St., Boston, Mass. (17).
Hawley, C. T., Spring Street Avenue, Milwaukee, Wis. (20).
Hayes, George E., Buffalo, N. Y. (15).
Herrick, Mrs. Sophie B., Baltimore, Md. (23).
Hervey, Rev. A. B., 10 North 2d St., Troy, N. Y. (22).
Hicks, John D., Agriculturist, Old Westbury, Queen's Co., L. I. (23.)
Hill, George W., Mathematician, Nyack Turnpike, New York (23).
Hills, S. W., Marshall, Mich. (6).
Hill, Rev. Dr. Thomas, 109 State St., Portland, Me. (3).
Hitchcock, Prof. Charles H., Hanover, N. H. (11).
Hoadley, Frederick H., Ass't in Yale Coll. Mus., New Haven, Conn. (23).
Holden, Prof. Edward S., Naval Observatory, Washington, D. C. (23).
Holley, George W., Niagara Falls, N. Y. (19).
Homes, Henry A., Librarian State Library, Albany, N. Y. (11).
Horr, Dr. Asa, Dubuque, Iowa (21).
Horrobin, William T., Cohoes, N. Y. (19).
Horsford, Prof. E. N., Cambridge, Mass. (1).
Hough, G. W., Albany, N. Y. (15).
Houk, Mrs. George W., Dayton, Ohio (22).
House, John C., Union Gas Works, Waterford, N. Y. (19).
Hovey, Prof. Edmund O , Wabash College, Crawfordsville, Ind. (20).
Hovey, Mrs. Edmund O., Crawfordsville, Ind. (21).
Hovey, Miss Mary F., Crawfordsville, Ind. (20).
Hoy, Dr. Philo R., Racine, Wis. (17).
Hubbard, Prof. Oliver P., 65 W. 19th Street, New York (1).
Hubbard, Mrs. Sara A., No. 31 Thirty-third St., Chicago, Ill. (17).
Hume, Mrs. A. L., Bombay, India (20).
Humphrey, D., M. D., Lawrence, Mass. (18).
Humphreys, A. W., Box 1384, N. Y. (20).
Hunt, Miss Sarah E , Salem, Mass. (20).
Huntington, Prof. J. H., Hanover, N. H. (19).
Hyatt, Prof. Alpheus, Natural History Society, Boston, Mass. (18).
Hyatt, Jonathan D., Mott Haven, N. Y. (19).

Irish, Thomas M., 1852 Valeria St., Dubuque, Iowa (21).

Jackson, Prof. C. L., care P. T. Jackson, Boston, Mass. (20).
Jackson, Lewis McL., Middletown, Conn. (22).
James, Thomas Potts, Cambridge, Mass. (22).
Jarvis, George C., M. D., Hartford, Conn. (23).
Jasper, Gustavus A., 12 Central St., Boston, Mass. (18).
Jewell, Pliny, Mineralogical Amateur, Hartford, Conn. (23).
Jillson, Dr. B. C., Pittsburgh, Penn. (14).
Johnson, Prof. Hosmer A., Academy of Sciences, Chicago, Ill. (22).
Johnson, Samuel M., Everett, Middlesex Co., Mass. (23).

Johnston, Prof. John, Middletown, Conn. (1).
Jones, Rev. Prof. William, Trinity College, Toronto, Canada (23).
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King, V. O., New Orleans, La. (21).
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Kneeland, Dr. Samuel, Mass. Institute of Technology, Boston, Mass. (20).
Knight, J. B., care Franklin Inst., Philadelphia, Penn. (21).

Langley, Prof. John Williams, Western Univ. of Pa., Pittsburgh, Pa.(23).
Lapham, Dr. Increase A., Chief of Geological Corps, Milwaukee, Wis. (3).
Lattimore, Prof. S. A., University of Rochester, Rochester, N. Y. (15).
Lawrence, Hon. Edw., Pres't Bunker Hill N. Bk., Charlestown, Mass. (18).
Lawrence, George N., 172 Pearl, cor. Pine St., New York (7).
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Lennon, W. H., Normal School, Brockport, N. Y. (19).
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Lewis, Elias, jr., 111 St. Mark's Ave., Brooklyn, N. Y. (23).
Lindsley, Dr. J. B., Nashville, Tenn. (1).
Lockwood, Rev. Samuel, Freehold, N. J. (18).
Logan, Sir William E., 15 St. Lambert St., Montreal, Canada (1).
Lovering, Prof. Joseph, Cambridge, Mass. (2).
Lupton, Prof. N. T., Vanderbilt Univ., Nashville, Tenn. (17).
Lyford, Prof. Moses, Waterville, Me. (22).
Lyman, Prof. Chester S., New Haven, Conn. (14).
Lyman, Col. Theodore, Brookline, Mass. (23).

MacArthur, Charles L., Troy, N. Y. (19).
Mack, Dr. William, Salem, Mass. (21).
Malone, David R., Edinburg, Ind. (20).

Mark, Edward L., Fredonia, N. Y. (21).
Martin, Prof. B. N., 236 W. 4th St., New York City (23).
Martin, Prof. Daniel S., Rutger's Female College, New York City (23).
McClintock, Frank, West Union, Iowa (22).
McCollister, Rev. S. H., Pres't Bucktel College, Akron, Ohio (22).
McMurtrie, Horace, Boston, Mass. (17).
McMurtrie, William, Dep't Agriculture, Washington, D. C. (22).
McRae, John, Camden, S. C. (3).
McWhorter, Tyler, Aledo, Ill. (20).
Means, Rev. A., Oxford, Ga. (5).
Meehan, Thomas, Germantown, Penn. (17).
Meek, F. B., Smithsonian Institution, Washington, D. C. (6).
Meigs, Dr. James Aitken, 423 South Broad St., Philadelphia, Penn. (12).
Mendenhall, Prof. T. C., Agri. and Mechanical Coll., Columbus, Ohio (20).
Merrill, Prof. George C., Washburn College, Topeka, Kansas (22).
Metcalf, Caleb B., Worcester, Mass. (20).
Miller, John A., Paducah, Ky. (22).
Milner, James W., Waukegan, Ill. (22).
Minifie, William, 114 Baltimore St., Baltimore, Md. (12).
Mitchell, Miss Maria, Vassar College, Poughkeepsie, N. Y. (4).
Moore, Prof. James W., M. D., Lafayette College, Easton, Penn. (22).
Moore, Joseph, Pres't Earlham Coll., Richmond, Ind. (20).
Morgan, Hon. L. H., Rochester, N. Y. (11).
Morison, Dr. N. H., Provost of Peabody Institute, Baltimore, Md. (17).
Morley, Prof. Edward W., Hudson, Ohio (18).
Morse, Prof. Edward S., Salem, Mass. (18).
Morton, Henry, Stevens' Inst. Tech., Hoboken, N. J. (18).
Muir, John, Oakland, Cal. (22).
Munroe, John C., Lexington, Mass. (22).
Munroe, William, 106 Boylston St., Boston, Mass. (18).

Nason, Almond F., 15 State St., Boston, Mass. (22).
Newberry, Prof. J. S., Cleveland, Ohio, and Columbia Coll., New York (5).
Newcomb, Mrs. Lydia B. Black, Teacher, New Haven, Conn. (23).
Newman, John S., 48 East Washington St., Indianapolis, Ind. (20).
Newman, Mrs. John S., 48 East Washington St., Indianapolis, Ind. (21).
Newton, Rev. John, Mary Esther, West Fla. (7).
Nichols, Charles A., Providence, R. I. (17).
Nichols, Prof. W. R., Mass. Inst. Technology, Boston, Mass. (18).
Nicholson, Dr. Thomas, 490 Magazine St., New Orleans, La. (21).
Northrop, Birdsey Grant, Sec. Conn. State Board of Education, New Haven, Conn. (23).
Norton, Miss Mary E. B., Rockford Seminary, Rockford, Ill. (21).
Norton, Prof. W. A., New Haven, Conn. (6).
Noyes, Henry D., M. D., 73 Madison Ave., New York City (23).
Nutt, Cyrus, Bloomington, Ind. (20).

Ogden, Mahlon D., Chicago, Ill. (17).
Ogden, Robert W., 44 Carondelet St., New Orleans, La. (21).
Ogden, W. B., High Bridge, Westchester County, N. Y. (17).
Oliver, Prof. James E., Cornell University, Ithaca, N. Y. (7).
Olmstead, Fred. Law, Commissioner of Public Parks, 209 W. 46th St., New York (22).
Ordway, John M., Mass. Inst. Tech., Boston, Mass. (9).
Orton, Prof. Edward, President Ohio Agricultural and Mechanical College, Columbus, Ohio (19).
Osborne, Amos O., Waterville, Oneida Co., N. Y. (19).

Packard, Dr. A. S., jr., Peabody Academy of Science, Salem, Mass. (16).
Paine, Charles, 163 Prospect St., Cleveland, Ohio (22).
Paine, Nathaniel, Worcester, Mass. (18).
Painter, Jacob, Institute of Science, Lima, Pa. (23).
Palmer, Rev. Benj. M., Box 1762, New Orleans, La. (21).
Palmer, Dr. Edward, care Smithsonian Inst., Washington, D. C. (22).
Palmer, Rev. James M., 74 Middle St., Portland, Me. (22).
Parker, Wilbur F., Ed. Am. Sportsman, West Meriden, Conn. (23).
Parry, Dr. Charles C., Davenport, Iowa (6).
Patton, William W., 380 W. Washington St., Chicago, Ill. (18).
Paul, Mrs. Caroline A., Vineland, Cumberland Co., New Jersey (23).
Peck, W. A., care Peck and Hillman, Troy, N. Y. (19).
Peckham, S. F., Univ. of Minn., Minneapolis, Minn. (18).
Pedrick, Wm. R., Lawrence, Mass. (22).
Peirce, Prof. Benjamin, Cambridge, Mass. (1).
Perkins, Prof. George H., Burlington, Vt. (17).
Perkins, Prof. George R., care Dr. Wm. H. Watson, Utica, N. Y. (1).
Perkins, Maurice, Schenectady, N. Y. (15).
Perkins, S. E., jr., Indianapolis, Ind. (20).
Perkins, T. Lyman, Salem, Mass. (22).
Phelps, Gen. Charles E., Baltimore, Md. (13).
Phelps, Mrs. Lincoln, Baltimore, Md. (13).
Philbrick, Prof. Philetus H., Iowa City, Iowa (23).
Pickering, Prof. Edward C., Boston, Mass. (18).
Pierce, Henry D., Indianapolis, Ind. (20).
Pillsbury, John H., Teacher of Nat. Sci., Wesleyan Acad., Wilbraham, Mass. (23).
Pond, Erasmus A., Rutland, Vt. (22).
Porteous, John, Agent Grand Trunk Railway, Portland, Me. (22).
Pourtales, L. F., Keeper Museum Comp. Zoology, Cambridge, Mass. (1).
Powell, J. W., Box 806, Washington, D. C. (23).
Pratt, William H., Davenport, Iowa (17).
Prescott, Prof. Albert B., Ann Arbor, Mich. (23).
Prince, Gen. Henry, Paymaster General of Coast Survey, New York (22).
Preston, W. C., Iowa City, Iowa (21).
Pruyn, John V. L., Chancellor Univ. of N.Y., 13 Elk St., Albany, N.Y. (1).
Pulsifer, Sidney, Peoria, Ill. (21).

Pumpelly, Prof. Raphael, Newburgh, Orange County, N. Y. (17).
Putnam, Mrs. F. W., Salem, Mass. (19).
Pynchon, Prof. Thomas R., Trinity College, Hartford, Conn. (23).

Rauch, Dr. J. H., Chicago, Ill. (11).
Raymond, R. W., Box 4404, New York, N. Y. (15).
Read, Ezra, Terre Haute, Ind. (20).
Remsen, Prof. Ira, Williams College, Williamstown, Mass. (22).
Richards, Prof. Robert H., Mass. Inst. of Technology, Boston, Mass. (22).
Richardson, F. C. A., Corner Garrison and Wash. Av., St. Louis, Mo. (20).
Ritchie, E. S., Boston, Mass. (10).
Robertson, Col. Robert S., Fort Wayne, Ind. (20).
Robinson, Henry C., Lawyer, Hartford, Conn. (23).
Robinson, Prof. Otis Hall, Rochester, N. Y. (23).
Rockwell, Alfred P., Office Board Fire Commissioners, Boston, Mass. (10).
Rockwell, Joseph P., Burlington, Iowa (17).
Roepper, Charles H., Bethlehem, Pa. (23).
Rogers, Prof. Robert E., University of Pa., Philadelphia, Penn. (18).
Rogers, W. A., Ass't Harv. Coll. Observatory, Cambridge, Mass. (15).
Rogers, Prof. William B., Hotel Berkeley, Boston, Mass. (1).
Rominger, Dr. Carl, Ann Arbor, Mich. (21).
Rood, Prof. O. N., New York (14).
Rose, Prof. P. B., Ann Arbor, Mich. (23).
Ross, Angus, Morris Street School, Dartmouth, Halifax Co., N. S. (22).
Rosseter, G. R., Marietta, Ohio (18).
Rowland, Prof. Henry A., Troy, N. Y. (23).
Runkle, Prof. J. D., Pres. Institute of Technology, Boston, Mass. (2).
Russell, Gurdon W., M. D., Hartford, Conn. (23).
Russell, L. W., Providence, R. I. (20).
Rutherford, Louis M., New York (13).

Sadtler, Prof. Samuel P., University of Penn., Philadelphia, Pa. (22).
Safford, James M., Nashville, Tenn. (6).
Sage, John H., Ornithologist, Portland, Conn. (23).
Sanders, Benjamin D., Wellsburg, Brooke County, W. V. (19).
Saunders, William, London, Canada (17).
Saunderson, Robert, Sup't of Public Schools, Burlington, Iowa (21).
Scammon, J. Young, Chicago, Ill. (17).
Schaack, Prof. J. Stillwell, Princeton College, Princeton, N. J. (4).
Schott, Charles A., Coast Survey Office, Washington, D. C. (8).
Seaman, Ezra C., Ann Arbor, Mich. (20).
Seely, Charles A., 26 Pine St., New York (18).
Senter, Harvey S., Aledo, Mercer Co., Ill. (20).
Seymour, Prof. William P., 105 Third St., Troy, N. Y. (19).
Shaler, Prof. N. S., Newport, Ky., and Cambridge, Mass. (19).
Sheafer, P. W., Pottsville, Penn. (4).
Sheldon, Edwin H., Chicago, Ill. (17).

Sill, J. M. B., Zoölogist, Detroit, Mich. (23).
Silliman, Prof. Justus M., Easton, Penn. (19).
Skinner, Joseph J., New Haven, Conn. (23).
Sloan, Dr. John, New Albany, Ind. (20).
Smith, Prof. Eugene A., University of Alabama, Tuscaloosa, Ala. (20).
Smith, James Y., 56 Westminster St., Providence, R. I. (9).
Smith, S. I., New Haven, Conn. (18).
Smock, Prof. John Conover, Rutger's Coll., New Brunswick, N. J. (23).
Snell, Prof. Ebenezer S., Amherst, Mass. (2).
Squier, Hon. E. G., 4 West Twenty-seventh St., New York (18).
Stanard, Benjamin A., Cleveland, Ohio (6).
Stearns, R. E. C., Oakland, Cal. (18).
Stearns, Winfrid Alden, Amherst, Mass. (23).
Stevens, Julius, Humboldt, Iowa (21).
Stevens, R. P., 24 Pine St., New York (18).
Stevens, Dr. Thaddeus M., Indianapolis, Ind. (20).
Steward, A., 155 State St., Chicago, Ill. (21).
Stimpson, Thomas M., Peabody, Mass. (18).
Stockwell, John N., 579 Case Av., Cleveland, Ohio (18).
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Storer, Dr. D. H., Boston, Mass. (1).
Storer, Prof. Frank H., Jamaica Plain, Mass. (13).
Storke, Helen L., Auburn, N. Y. (19).
Storrs, Henry E., Jacksonville, Ill. (20).
Stowell, John, 48 Main Street, Charlestown, Mass. (21).
Sutton, George, Aurora, Ind. (20).
Swain, James, Fort Dodge, Iowa (21).
Swain, Mrs. James, Fort Dodge, Iowa (21).
Swallow, Miss Ellen H., Mass. Inst. Tech., Boston, Mass. (23).
Swallow, Prof. G. C., Columbia, Mo. (10).
Swan, S. E. (22).
Swasey, Oscar F., M. D., Beverly, Mass. (17).

Taft, Prof. S. H., President Humboldt College, Humboldt, Iowa (21).
Taft, Mrs. S. H., Humboldt, Iowa (21).
Talbot, Hon. George F., Portland, Me. (22).
Tappan, Eli T., Pres't of Kenyon College, Gambier, Ohio (20).
Tenney, Prof. Sanborn, Williamstown, Mass. (17).
Tewksbury, Samuel H., Portland, Me. (22).
Thompson, Aaron R., 36 Pine St., New York (1).
Thompson, Harvey M., Box 179, Chicago, Ill. (17).
Thompson, Joseph P., Portland, Me. (22).
Thompson, Robert H., Troy, N. Y. (19).
Thrasher, William M., Indianapolis, Ind. (21).
Thurber, Miss Elizabeth, Plymouth, Mass. (22).
Thurston, Prof. Robert H., Stevens Inst., Hoboken, N. J. (23).

Tillman, Mrs. S. D., Jersey City, N. J. (20).
Todd, Prof. James E., Tabor, Fremont Co., Iowa (22).
Tolles, Robert B., 40 Hanover St., Boston, Mass. (15).
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Toner, Joseph M., M. D., Washington, D. C. (23).
Townsend, Hon. Franklin, Albany, N. Y. (4).
Townshend, Prof. N. S., Columbus, Ohio (17).
Trembley, Dr. J. B., San Jose, Santa Clara Co., Cal. (17).
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Verrill, Prof. A. E., Yale College, New Haven, Conn. (16).
Vose, Prof. George L., Bowdoin College, Brunswick, Me. (15).

Wadsworth, M. E., Instructor Lawr. Sci. Sch., Cambridge, Mass. (23).
Walker, Charles A., 42 Court St., Boston, Mass. (18).
Walker, George C., 228 Michigan Ave., Chicago, Ill. (17).
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Wanzer, Ira, Brookfield, Conn. (18).
Ward, Prof. Henry A., Rochester, N. Y. (13).
Warder, Robert B., Cleves, Hamilton Co., Ohio (19).
Wardwell, George J., Rutland, Vt. (20).
Warner, Mrs. J. D., 199 Baltic St., Brooklyn, N. Y. (21).
Warren, Gen. G. K., U.S.A., Engineer's Office, Newport, R. I. (12).
Warren, G. W., 42 Court St., Boston, Mass. (18).
Warren, Prof. S. Edward, Institute of Technology, Boston, Mass. (17).
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Wead, Prof. Charles K., Pittsburgh, Pa. (23).
Webb, Benjamin, jr., Salem, Mass. (18).
Webster, Prof. H. E., Union College, Schenectady, N. Y. (23).
Weld, W. G., Boston, Mass. (23).
Wells, Daniel H., New Haven, Conn. (18).
Wells, George A., Troy, N. Y. (19).
Westcott, O. S., High School, Chicago, Ill. (21).
Wheatley, Charles M., Phœnixville, Penn. (1).
Wheeler, C. G., Chicago, Ill. (18).
Wheeler, Dr. T. B., Box 38½, Montreal, Canada (11).

Wheelock, G. A., Keene, N. H. (22).
White, Prof. C. A., Bowdoin College, Brunswick, Me. (17).
White, Lt. H. C., U. S. Navy (23).
White, Leroy S., Waterbury, Conn. (23). [Phila., Pa. (23).
White, Samuel S., Publisher Dental Cosmos, Chestnut St., cor. Twelfth,
Whitfield, R. P., Albany, N. Y. (18).
Whitney, Mary W., Waltham, Mass. (19).
Whittlesey, Col. Charles, Cleveland, Ohio (1).
Wilder, Dr. Burt G., Cornell University, Ithaca, N. Y. (22).
Williams, H. S., Williams Brothers, Phenix Iron Works, Ithaca, N. Y. (18).
Williamson, Lieut. Col. R. S., U. S. Eng., Light House Engineer, San Francisco, Cal. (12).
Winchell, Prof. N. H., St. Anthony, Minn. (19).
Woodman, H. T., Dubuque, Iowa (20). [D. C. (17).
Woodworth, Dr. John M., U. S. Marine Hospital Service, Washington,
Worster, Joseph, M. D., 115 East Thirtieth St., New York (22).
Wright, A. W., Yale College, New Haven, Conn. (14).
Wurtele, Rev. Louis C., Acton Vale, Province of Quebec, Canada East (11).
Wurtz, Henry, 12 Hudson Terrace, Hoboken, N. J. (10).
Wylie, Prof. Theophilus A., Ind. State University, Bloomington, Ind. (20).

Yarrow, Dr. H. C., U. S. Engineer Office, Washington, D. C. (23).
Young, Prof. Charles A., Dartmouth College, Hanover, N. H. (18).
Young, William H., 8 and 9 First St., Troy, N. Y. (19).

[368 MEMBERS.]

FELLOWS.

Alvord, Benj., U. S. A., Paymaster General, Washington, D. C. (17).
Atkinson, Prof. Wm. H., 41 East 9th St., New York City (22).

Bailey, W. W., Teacher of Botany, Providence, R. I. (18).
Bardwell, Prof. F. W., University of Kansas, Lawrence, Kan. (13).
Barnard, F. A. P., Pres't Columbia College, New York City (7).
Batchelder, Dr. J. H., Salem, Mass. (18).
Bell, Samuel N., Manchester, N. H. (7).
Bicknell, Edwin, Cambridge, Mass. (18).
Blake, Eli W., New Haven, Conn. (1).
Blake, Prof. Eli W., jr., Providence, R. I. (15).
Bross, Hon. Wm., Chicago, Ill. (7).
Brown, Robert, jr., Office Cincinnati Gas Light Co., Cincinnati, Ohio (11).
Brown, Mrs. Robert, jr., Cincinnati, Ohio (17).
Brush, Prof. George J., Yale College, New Haven, Conn. (11).
Bushee, Prof. James, Worcester, Mass. (9).

Capen, Rev. Francis L., care B. Capen, Esq., City Hall, Boston, Mass. (23).
Carrier, Prof. Joseph C., Notre Dame, Ind. (20).
Chandler, Prof. W. H., the Lehigh University, Bethlehem, Pa. (19).
Clarke, Prof. F. W., Cincinnati University, Cincinnati, Ohio (18).
Coffin, Prof. John H. C., U. S. N., Washington, D. C. (1).
Coffin, Prof. Selden J., Lafayette College, Easton, Pa. (22).
Colbert, Elias, Editor Chicago Tribune, Chicago, Ill. (17).
Collett, John, Newport, Ind. (17).
Cox, Prof. Edward T., Indianapolis, Ind. (19).
Craik, Robert, M. D., McGill University, Montreal, Canada (11).
Cramp, Rev. J. M., D. D., Wolfville, N. S. (11).
Cutting, Dr. Hiram A., Lunenburgh, Vt. (17).

Dall, Mrs. Caroline H., 141 Warren Ave., Boston, Mass. (18).
Dall, Wm. H., Box 1869, San Francisco, Cal. (18).
Dalrymple, Rev. Dr. E. A., Baltimore, Md. (11).
Danforth, Edward, Dep't of Public Instruction, Albany, N. Y. (11).
Day, F. H., M. D., Wauwatosa, Wis. (20).
Dimmock, George, Springfield, Mass. (22).
Dwyer, Dr. John, Hartford, Conn. (23).

Eaton, Prof. D. G., Packer Institute, Brooklyn, N. Y. (19).
Edwards, Thomas C., P. O. Box A. E. Vineland, N. J. (21).
Eimbeck, Wm., U. S. C. S., P. O. Box 1600, San Francisco, Cal. (17).
Elliott, Ezekiel B., Statistical Bureau, Washington, D. C. (10).

Elmore, Samuel E., Pres't Conn. River Banking Co., Hartford, Conn. (23).
Elsberg, Louis, M. D., 8 West 28th St., New York, N. Y. (23).
Eustis, Prof. Henry L., Cambridge, Mass. (2).
Evans, Asher B., Principal Union School, Lockport, N. Y. (19).

Fairbanks, Prof. Henry, St. Johnsbury, Vt. (14).
Fitch, Edward H., Ashtabula, Ohio (11).
Foote, Dr. A. E., Central Park Museum, New York City (21).

Gardiner, Rev. Frederic, D. D., Middletown, Conn. (23).
Gilbert, G. K., 99 Arcade, Rochester, N. Y. (18).
Gleason, F. L., Manufacturer Iron and Steel, Hartford, Conn. (23).
Green, Dr. Traill, Easton, Pa. (1).
Grimes, J. Stanley, Evanston, Ill. (17).

Haldeman, Prof. S. S., Chickis, Pa. (1).
Hale, Dr. William H., Albany, N. Y. (19).
Hambly, John B., Portsmouth, R. I. (18).
Hamlin, Dr. A. C., Bangor, Me. (10).
Hartshorne, Prof. Henry, Haverford College, Montgomery Co., Penn. (12).
Hasbrouck, Prof. Isaac E., Rutger's College, New Brunswick, N. J. (23).
Hedrick, B. S., Examiner U. S. Patent Office, Washington, D. C. (19).
Henry, Prof. Joseph, Secretary Smithsonian Institution, Washington, D. C. (1).
Hilgard, Prof. Eugene W., Univ. of Cal., Berkeley, Cal. (1).
Hilgard, Prof. Julius E., U. S. Coast Survey, Washington, D. C. (4).
Hinrichs, Prof. Gustavus, State University, Iowa City, Iowa (17).
Hosford, Charles E., Terre Haute, Ind. (20).
Hough, Franklin B., Lowville, N. Y. (4).
Howe, Elliott C., M. D., Yonkers, N. Y. (19).
Hunt, George, Providence, R. I. (9).
Hunt, Dr. T. Sterry, St. James Hotel, Boston, Mass. (1).
Hyatt, James, Stanfordville, Dutchess Co., N. Y. (10).

Jenks, Elisha T., Middleborough, Mass. (22).
Jenks, Prof. J. W. P., Middleborough, Mass. (2).
Johnson, S. W., Sheffield Scientific School, New Haven, Conn. (22).

Kellogg, Justin, 259 River St., Troy, N. Y. (19).
Kerr, Prof. W. C., Raleigh, N. C. (10).
King, Robert, Kalamazoo, Mich. (21).
Kirkwood, Prof. Daniel, Bloomington, Ind. (7).

Lambert, Thomas R., Charlestown, Mass. (18).
Lambert, Dr. T. S., New York City (21).
Langley, S. P., Director Observatory, Allegheny, Pa. (18).
Laws, Samuel S., Teacher and Student Gen. Sci., New York City (23).

LeConte, Dr. John L., 1625 Spruce St., Philadelphia, Pa. (1).
Leeds, Prof. Albert R., Stevens Institute, Hoboken, N. J. (23).
Lesley, Prof. J. Peter, State Geol. of Pa., 1008 Clinton St., Phil., Pa. (2).
Levette, Gilbert M., Geologist, Indianapolis, Ind. (23).
Lintner, J. A., N. Y. State Museum of Nat. Hist., Albany, N. Y. (22).
Little, W. C., Albany, N. Y. (22).
Loomis, Prof. Elias, New Haven, Conn. (1).
Loughridge, Prof. R. H., Geol., Dep't Agric., Atlanta, Ga. (21).

Mann, B. Pickman, 19 Follen St., Cambridge, Mass. (22).
Marcy, Prof. Oliver, Evanston, Ill. (10).
Marsh, Prof. O. C., Yale College, New Haven, Conn. (15).
Mayer, Prof. Alfred M., Stevens Inst. Technology, Hoboken, N. J. (19).
McRae, Hamilton S., Sup't of Schools, Muncie, Ind. (20).
Morris, Rev. John G., Baltimore, Md. (12).
Munroe, Prof. Chas. E., U. S. Naval Acad., Annapolis, Md. (22).

Nason, Prof. H. B., Rensselaer Polytechnic Inst., Troy, N. Y. (13).
Nelson, The Rev. Dr. Cleland K., Annapolis, Md. (12).
Newcomb, Prof. Simon, U. S. Naval Observatory, Washington, D. C. (13).
Newton, Hubert A., New Haven, Conn. (6).
Niles, Prof. W. H., Cambridge, Mass. (16).

Oliver, Miss Mary E., Cascadilla Hotel, Ithaca, N. Y. (20).
Osborne, John W., Washington, D. C. (22).
Owen, Dr. Richard, Ind. State Univ., Bloomington, Ind. (20).

Paine, Cyrus F., Rochester, N. Y. (12).
Palfray, Hon. Charles W., Salem, Mass. (21).
Parkhurst, Henry M., Law Stenographer, 33 Park Row, New York (23).
Peirce, B. O., 413 Broadway, Cambridge, Mass. (18).
Phippen, Geo. D., Salem, Mass.(18).
Putnam, F. W., Director of Peabody Acad. of Science, Salem, Mass. (10).

Quimby, Prof. E. T., Hanover, N. H. (22).
Quincy, Edmund, jr., 160 South Market St., Boston, Mass. (11).

Redfield, John H., care A. Whitney & Sons, Philadelphia, Pa. (1).
Rice, Prof. Wm. North, Middletown, Conn. (18).
Riley, Prof. C. V., St. Louis, Mo. (17).
Rockwood, Prof. Charles G., Jr., Rutger's Coll., New Brunswick, N. J.(20).
Rogers, Fairman, 202 West Rittenhouse Sq., Philadelphia, Pa. (11).
Roosevelt, Clinton, No. 11 Wall St., N. Y. (11).
Ross, Dr. Alexander M., Toronto, Ontario (21).

Saville, Dr. John J., U. S. Indian Agent, Red Cloud Agency, D. T. (22).
Scudder, Samuel H., Cambridge, Mass. (13).

Seaman, Wm. H., Microscopist, Agric. Dep't, Washington, D. C. (23).
Silas, Solomon, Schoharie C. H., Schoharie Co., N. Y. (10).
Sill, Hon. Elisha N., Cuyahoga Falls, Ohio (6).
Silliman, Prof. Benjamin, Yale Coll., New Haven, Conn. (1).
Smith, J. Lawrence, Louisville, Ky. (1).
Spencer, John W., Teacher and Geologist, Paxton, Ind. (20).
Stearns, Henry P., M. D., Hartford, Conn. (23).
Stelner, Dr. Lewis H., Frederick City, Md. (7).
Stephens, W. Hudson, Lowville, N. Y. (18).
Stetson, Thomas D., Solicitor of Patents, 155 East 71st St., New York (23).
Stuart, Prof. A. P. S., Clinton, Mass. (21).

Terry, N. M., U. S. Naval Academy, Annapolis, Md. (23).
Tillman, Prof. Samuel D., Jersey City, N. J. (15).
Trowbridge, Prof. W. P., New Haven, Conn. (10).
Tuttle, Albert H., Columbus, Ohio (17).

Uhler, Philip R., Baltimore, Md. (19).

Van der Weyde, Dr. P. H., New York City (17).

Walker, Prof. Joseph B., care Bank of Kentucky, Louisville, Ky. (20).
Walker, Prof. J. R., Napoleon Ave., cor. Coliseum St., New Orleans, La. (19).
Waller, Elwyn, School of Mines, Columbia Coll., New York City (23).
Ward, Dr. Henry D. A., Meteorologist, Middletown, Conn. (23).
Ward, Dr. R. H., 53 Fourth St., Troy, N. Y. (17).
Warner, James D., 199 Baltic St., Brooklyn, N. Y. (18).
Webster, Prof. Nathan B., Principal Webster Inst., Norfolk, Va. (7).
Wheatland, Dr. Henry, Pres't Essex Institute, Salem, Mass. (1).
Wheildon, W. W., Concord, Mass. (13).
Whitman, Wm. E., Philadelphia, Pa. (23).
Whitney, Solon F., Watertown, Mass. (20).
Wilber, G. M., Pine Plains, N. Y. (19).
Wilbur, A. B., Port Jervis, Orange Co., N. Y. (23).
Wiley, Prof. Harvey, W., M. D., Purdue University, La Fayette, Ind. (21).
Williams, Charles H., M. D., 15 Arlington St., Boston, Mass. (22).
Williams, Henry W., M. D., 15 Arlington St., Boston, Mass. (11).
Worthen, A. H., Springfield, Ill. (5).
Wyckoff, Wm. C., Tribune Office, New York City (20).

Youmans, Prof. Edward L., New York City (6).

MEMBERS ELECTED

AT

HARTFORD MEETING.

One hundred and eighteen members were elected at the Hartford Meeting. Of these one hundred and eight have paid the admission fee and assessment for the meeting, and their names have been incorporated into the list of Members. Two have declined, and the following have not yet replied to the notifications sent to them:

Bikle, Prof. Philip M., Gettysburg, Pa.
Felt, George Henry, 32 Liberty St., New York City.
Fowler, William W., Durham, Conn.
Levison, W. Gorld, 314 Livingston St., Brooklyn, L. I.
Price, Isah B., Union University, Schenectady, N. Y.
Singleton, William R., Washington, D. C.
Thorburn, W. H. S., School of Mines, Columbia College, New York City.
Treadwell, Howard, Albany College, Albany, N. Y.

DECEASED MEMBERS.

Abert, J. J., Washington, D. C. (1).
Adams, C. B., Amherst, Mass. (1).
Adams, Edwin F., Charlestown, Mass. (18).
Agassiz, Louis, Cambridge, Mass. (1).
Ainsworth, J. G., Barry, Mass. (14).
Allston, R. F. W., Georgetown, S. C. (3).
Anthony, Charles H., Albany, N. Y. (6).
Ames, M. P., Springfield, Mass. (1).
Appleton, Nathan, Boston, Mass. (1).

Bache, Alexander D., Washington, D. C. (1).
Bache, Franklin, Philadelphia, Pa. (1).
Bailey, J. W., West Point, N. Y. (1).
Barrett, Moses, Milwaukee, Wis. (21).
Beck, C. F., Philadelphia, Penn. (1).
Beck, Lewis C., New Brunswick, N. J. (1).
Beck, T. Romeyn, Albany, N. Y. (1).
Benedict, G. W., Burlington, Vt. (16).
Binney, Amos, Boston, Mass. (1).
Binney, John, Boston, Mass. (3).
Blanding, William, R. I. (1).
Blatchford, Thomas W., Troy, N. Y. (6).
Blatchley, Miss S. L., New Haven, Conn. (19).
Bomford, George, Washington, D. C. (1).
Braithwaite, Jos., Chambly, C. W. (11).
Brown, Andrew, Natchez, Miss. (1).
Burnap, G. W., Baltimore, Md. (12).
Burnett, Waldo I., Boston, Mass. (1).
Butler, Thomas B., Norwalk, Conn. (10).

Carpenter, Thornton, Camden, S. C. (7).
Carpenter, William M., New Orleans, La. (1).
Case, William, Cleveland, Ohio (6).
Chapman, N., Philadelphia, Pa. (1).
Chase, S., Dartmouth, N. H. (2).
Chauvenet, William, St. Louis, Mo. (1).
Clapp, Asahel, New Albany, Ind. (1).
Clark, H. J., Cambridge, Mass. (13).
Clark, Joseph, Cincinnati, Ohio (5).

Clarke, A. B., Holyoke, Mass. (13).
Cleaveland, C. H., Cincinnati, Ohio (9).
Cleveland, A. B., Cambridge, Mass. (2).
Coffin, James H., Easton, Penn. (1).
Cole, Thomas, Salem, Mass. (1).
Coleman, Henry, Boston, Mass. (1).
Cooper, William, Hoboken, N. J. (9).
Couper, J. Hamilton, Darien, Ga. (1).
Corning, Erastus, Albany, N. Y. (6).
Crosby, Alpheus, Salem, Mass. (10).
Crosby, Thomas R., Hanover, N. H. (18).
Croswell, Edwin, Albany, N. Y. (6).
Curry, W. F., Geneva, N. Y. (11).

Dayton, Edwin A., Madrid, N. Y. (7).
Dean, Amos, Albany, N. Y. (6).
Dearborn, George H. A. S., Roxbury, Mass. (1).
Dekay, James E., New York (1).
DeLaski, John, Carver's Harbor, Me. (18).
Dewey, Chester, Rochester, N. Y. (1).
Dexter, G. M., Boston, Mass. (11).
Dillingham, W. A. P., Augusta, Me. (17).
Doolittle, L., Lenoxville, C. E. (11).
Ducatel, J. T., Baltimore, Md. (1).
Duffield, George D.D., Detroit, Mich. (10).
Dumont, A. H., Newport, R. I. (14).
Duncan, Lucius C., New Orleans, La. (10).
Dunn, R. P., Providence, R. I. (14).

Easton, Norman, Fall River, Mass. (14).
Ely, Charles Arthur, Elyria, Ohio (4).
Emmons, Ebenezer, Williamstown, Mass. (1).
Engstrom, A. B., Burlington, N. J. (1).
Everett, Edward, Boston, Mass. (2).
Ewing, Thomas, Lancaster, Ohio (5).

Ferris, Dr. Isaac, New York (6).
Fillmore, Millard, Buffalo, N. Y. (7).
Fisher, Mark, Trenton, N. J. (10).
Fitch, Alexander, Hartford, Conn. (1).
Forbush, E. B., Buffalo, N. Y. (15).
Force, Peter, Washington, D. C. (4).
Foster, J. W., Hyde Park, Chicago, Ill. (1).
Foucon, Felix, Madison, Wis. (18).
Fowle, William B., Boston, Mass. (1).
Fox, Charles, Grosse Ile, Mich. (7).
Frazer, John F., Philadelphia, Pa. (1).
French, J. W., West Point, N. Y. (11).

Gavit, John E., New York (1).
Gay, Martin, Boston, Mass. (1).
Gibbon, J. H., Charlotte, N. C. (3).
Gillespie, W. M., Schenectady, N. Y. (10).
Gilmor, Robert, Baltimore, Md. (1).
Gould, Augustus A., Boston, Mass. (11).
Gould, B. A., Boston, Mass. (2).
Graham, James D., Washington, D. C. (1).
Gray, Alonzo, Brooklyn, N. Y. (13).
Gray, James H., Springfield, Mass. (6).
Greene, Benjamin D., Boston, Mass. (1).
Greene, Samuel, Woonsocket, R. I. (9).
Greer, James, Dayton, Ohio (20).
Griffith, Robert E., Philadelphia, Penn. (1).
Guest, William E., Ogdensburg, N. Y. (6).

Hackley, Charles W., New York (4).
Hale, Enoch, Boston, Mass. (1).
Hare, Robert, Philadelphia, Penn. (11).
Harlan, Joseph G., Haverford, Penn. (8).
Harlan, Richard, Philadelphia, Penn. (1).
Harris, Thaddeus W., Cambridge, Mass. (1).
Harrison, Jos. jr., Philadelphia, Pa. (12)
Hart, Simeon, Farmington, Conn. (1).
Haven, Joseph, Chicago, Ill. (17).
Hayden, H. H., Baltimore, Md. (1).
Hayward, James, Boston, Mass. (1).
Hilgard, Theo. C., St. Louis, Mo. (17).
Hickox, S. V. R., Chicago, Ill. (17).
Hitchcock, Edward, Amherst, Mass. (1).
Hodgson, W. B., Savannah, Ga. (10).
Holbrook, J. E., Charleston, S. C. (1).
Hopkins, Albert, Williamstown, Mass. (19).
Hopkins, James G., Ogdensburg, N. Y. (10).
Hopkins, T. O., Williamsville, N. Y. (10).
Hosford, Benj. F., Haverhill, Mass. (13).
Horton, William, Craigville, Orange Co., N. Y. (1).
Horton, C. V. R., Chaumont, N. Y. (10).
Houghton, Douglas, Detroit, Mich. (1).
Howland, Theodore, Buffalo, N. Y. (15).
Hubbert, James, Richmond, Province of Quebec (16).
Hunt, E. B., Washington, D. C. (2).
Hunt, Freeman, New York (11).

Ives, Moses B., Providence, R. I. ( ).
Ives, Thomas P., Providence, R. I. (10).

Johnson, W. R., Washington, D. C. (1).
Jones, Catesby A. R., Washington, D. C. (8).

Keep, N. C., Boston, Mass. (13).
Kennicott, Robert, West Northfield, Ill. (12).
King, Mitchell, Charleston, S. C. (3).

Lasel, Edward, Williamstown, Mass. (1).
Lederer Baron von, Washington, D. C. (1).
Lieber, Oscar M., Columbia, S. C. (8).
Lincklaen, Ledyard, Cazenovia, N. Y. (1).
Linsley, James H., Stafford, Conn. (1).
Loosey, Charles F., New York (12).
Lothrop, Joshua R., Buffalo, N. Y. (15).
Lyon, Sidney S., Jeffersonville, Ind. (20).

Maack, G. A., Cambridge, Mass. (18).
Marsh, Dexter, Greenfield, Mass. (1).
Mather, William W., Columbus, Ohio (1).
M'Conihe, Isaac, Troy, N. Y. (4).
Meade, George G., Philadelphia, Pa. (15).
Mitchell, Wm. H., Florence, Ala. (17).
Morton, S. G., Philadelphia, Penn. (1).

Newton, E. H., Cambridge, N. Y. (1).
Nicollett, J. N., Washington, D. C. (1).
Norton, J. P., New Haven, Conn. (1).
Noyes, J. O., New Orleans, La. (21).

Oakes, William, Ipswich, Mass. (1).
Olmsted, Alexander F., New Haven, Conn. (4).
Olmsted, Denison, New Haven, Conn. (1).
Olmsted, Denison, Jr., New Haven, Conn. (1).

Painter, Minshall, Lima, Pa. (7).
Parkman, Samuel, Boston, Mass. (1).
Perkins, Henry C., Newburyport, Mass. (18).
Perry, John B., Cambridge, Mass. (16).
Perry, M. C., New York (10).
Plumb, Ovid, Salisbury, Conn. (9).
Pope, Charles A., St. Louis, Mo. (12).
Porter, John A., New Haven, Conn. (14).
Pugh, Evan, Centre Co., Penn. (14).

Redfield, William C., New York (1).
Rockwell, John A., Norwich, Conn. (10).
Rogers, James B., Philadelphia, Penn. (1).

Seward, William H., Auburn, N. Y. (1).
Silliman, Benjamin, New Haven, Conn. (1).
Smith, J. V., Cincinnati, Ohio (5).
Smith, Lyndon A., Newark, N. J. (9).
Sparks, Jared, Cambridge, Mass. (2).
Stimpson, William, Chicago, Ill. (12).
Sullivant, W. S., Columbus, Ohio (7).

Tallmadge, James, New York (1).
Taylor, Richard C., Philadelphia, Penn. (1).
Teschemacher, J. E., Boston, Mass. (1).
Thompson, Z., Burlington, Vt. (1).
Thurber, Isaac, Providence, R. I. (9).
Torrey, John, New York (1).
Totten, J. G., Washington, D. C. (1).
Townsend, John K., Philadelphia, Penn. (1).
Troost, Gerard, Nashville, Tenn. (1).
Tuomey, M., Tuscaloosa, Ala. (1).
Tyler, Edward R., New Haven, Conn. (1).

Vancleve, John W., Dayton, Ohio (1).
Vanuxem, Lardner, Bristol, Penn. (1).

Wadsworth, James S., Genesee, N. Y. (2).
Wagner, Tobias, Philadelphia, Penn. (9).
Walker, Joseph, Oxford, N. Y. (10).
Walker, Sears C., Washington, D. C. (1).
Walker, Timothy, Cincinnati, Ohio (4).
Walsh, Benjamin D., Rock Island, Ill. (17).
Warren, John C., Boston, Mass. (1).
Webster, H. B., Albany, N. Y. (1).
Webster, J. W., Cambridge, Mass. (1).
Webster, M. H., Albany, N. Y. (1).
Wheatland, Richard H., Salem, Mass. (13).
Whitney, Asa, Phila., Penn. (1).
Willard, Emma, Troy, N. Y. (15).
Wilson, W. C., Carlisle, Pa. (12).
Woodbury, L., Portsmouth, N. H. (1).
Wright, John, Troy, N. Y. (1).
Wyman, Jeffries, Cambridge, Mass. (10).

Young, Ira, Hanover, N. H. (7).

ADDRESS

OF

PROFESSOR JOSEPH LOVERING,

THE RETIRING PRESIDENT OF THE ASSOCIATION.

Gentlemen and Ladies of the American Association for the Advancement of Science:—

When the States General of France were assembled for the last time at Versailles, after a long interval of inactivity, and an inaugural address was pronounced by the Bishop of Nancy, Mirabeau passed upon his performance the sweeping criticism that he had missed the grandest opportunity ever offered to man for saying something or holding his tongue. And, whenever this Association, comprising not only those who teach, but many who create science, assembles, as it now does, to listen to the address of its retiring President, if he is duly sensible of his responsibility, he would gladly avail himself of Mirabeau's alternative, either of being equal to the occasion or of being silent. But the rule of the Association, adopted in the original draft of the constitution at Philadelphia, and the example of my predecessors which I am unwilling to reverse, leave me no choice; and when I see around me, not the terrible monsters of the French revolution, maddened by the miseries of a downtrodden country, but calm and high-minded lovers of truth, I feel sure of a just and generous criticism. Welcome, then, the precious opportunity, enjoyed by the President

of this Association, of discussing some of the great themes of science before an audience which has for its nucleus the original investigators, discoverers, and inventors in the country, and which like the sun, is surrounded by an extensive chromosphere only a little less brilliant than the central body by contrast; and let my earnest endeavor be not to abuse or waste the great privilege.

I am confronted on the very threshold of my address by the doubt whether it were better to beat out the little bit of golden thought, for which I have time and capacity, into a thin leaf which shall merely gild the whole vast surface of scientific investigation, even for a single year, or to condense it into a solid though minute globule, only big enough and bright enough to light up some narrow specialty. The general practice which prevails, of selecting a President alternately from the two principal sections into which the Association is divided, will justify me in paying my particular addresses to the physical sciences, knowing that the large and active department of Natural History will be properly treated in its turn by those most competent to do it. Not even the capacious mind of a Goethe, a Humboldt, a Whewell, or a Herbert Spencer is large enough to give a decent shelter to all the subjects which come within the scope of this Association. At the same time I must say that I sympathize with the remarks made by President Hunt at Indianapolis, when he questioned the propriety of excluding geology from the ranks of the physical sciences; only I would give them a still wider significance. Physical science is distinguished from natural history not so much by its subjects as its methods. In my imagination I can picture to myself all these subjects as being handled in the same masterly grasp of mechanics and mathematics by which the physical astronomer holds in his hands the history and the destiny of the solar system. What is only a dream or a fancy now may become a reality to the science of the future. Why, asked Cuvier, may not natural history some day have its Newton: to whom the laws of circulation of the sap and the blood will be only as the laws of Kepler. With such an endorser, I may venture to quote these words of a consummate mathematician without fear of their being cast aside by the naturalists as one of Bacon's Idols of the Tribe. "An intelligence which at any given instant should know all the forces by which nature is urged and the respective situations of the beings of which nature is composed, if, moreover, it were suffic-

iently comprehensive to subject these data to calculation, would include in the same formula the movements of the largest bodies of the universe and those of the slightest atom. Nothing would be uncertain to such an intelligence, and the future no less than the past would be present to its eyes." The time has already come when a knowledge of physical laws and familiarity with the instruments of physical research are indispensable to the naturalist. I would not recommend that dissipation of intellectual energy, which will make a man superficial in all the sciences but profound in none. But Helmholtz has established, by his own example, the possibility of being an eminent physiologist and, at the same time, standing in the front rank of physicists and mathematicians. The restlessness of human inquiry will never be satisfied with knowing what things are, until it has also discovered how and why they are, and until all the relations of space, time, matter, and force, in all the kingdoms of nature, have been worked out with mathematical precision.

It is a happy circumstance in the history of science, that this vast mechanical problem did not rush upon the mind at once in all its crushing generality. The solar system, with a despotic sun at the centre, competent to overrule all insubordination among planets and comets and check all eccentricities and jealousies, and so far isolated from neighbouring systems as to fear nothing from foreign interferences and entangling alliances, presented a comparatively simple problem: and yet the skill and labor of many generations of mathematicians have not yet closed up the argument upon this first case. On the orbits of this domestic system they have been sharpening their tools for higher and more delicate work. The motions of binary stars have also been brought under dynamical laws, and partially subjected to the rule of gravitation, so far as the astronomer can judge from the best observations which he can make upon those remote objects. But when he launches out, with his instruments and his formulas, into clusters of stars, even those of greatest symmetry, he is wholly at sea, without chart or compass or lighthouse, and with no other illumination than that which comes from a prophetic demonstration in Newton's Principia. The mathematician has here to treat, not with an unlimited monarchy, as in the solar system, but with a republic of equal stars, and the dynamical condition of the clusters is involved in all the obscurity of molecular mechanics; for

it matters not whether the individual members of a system are atoms or worlds, if the intervening spaces have corresponding magnitudes. Even in astronomy, the inspiration of mechanics and the pride of mathematics, how trifling is the region which has been subjugated to the rigid rules of the exact sciences when compared with the immense territories which remain under the jurisdiction of natural history, and must be studied, if at all, by the methods of the naturalist, though with an inverted microscope.

If now we circumscribe our outlook by the line which marks where physical science ends and natural history begins, it will be possible to examine only a few of the salient points in the prospect before us: and what these are will depend upon the point of view which we select. Whewell presents the history of any science at each of its successive epochs as circulating around one powerful mind, which figures as the hero of the drama: and whatever immediately precedes or follows is only the prelude or the closing strain to the great movement. In the philosophy of Comte, every science passes through a theological and metaphysical crisis before it reaches the healthy condition of positive knowledge, and its whole history is written out by him in these three acts. With Buckle, the progress of science, without which there could be no history, is coincident with the advance in civilization; but the action begins with science, and the reaction only comes from external causes. All that science and civilization demand is perfect freedom of thought. The worst enemy of both is the protective spirit in church and state, the former telling men what they must believe, the latter what they must do.

Each of these views of scientific development may be true but not to the exclusion of all others. Metaphysical blindness or theological prejudice may block the way of science or defame its fair name. It has been stated that six members of the ultraclerical party at Versailles voted against the appropriation for securing observations of the approaching transit of Venus, because they did not believe in the Copernican system, and this too while the echoes of the celebration of the four hundredth birth-day of Copernicus are still resounding over the earth. So also, circumstances and even accidents may shape the course of discovery: the happiest of all accidents, however, being the appearance on the stage of the discoverer himself.

The point of view which I have chosen for reviewing the close

and advancing columns of the physical sciences is this:—Are there any improvements in the weapons of attack, or have any additions been made to them? These are of two kinds:—1. Instruments for experiment, and 2. The logic of mathematics. These are the lighter and the heavier artillery in this peaceful service.

If we cast a hurried glance over that long period of experimental research which began with Galileo and ended with Davy, we recognize, as the chief instrumentalities by which physical science has been promoted, the telescope, the microscope, the pendulum, the balance, and the voltaic battery. It is not necessary for me to enlarge upon the strength and accuracy which the battery and the balance have given to chemistry, or on the stretch and precision of vision which the telescope and microscope have bestowed on astronomy and physics. These instruments, the veterans of many a hard fought battle, science still enjoys: not superannuated by their long service but continually growing in power and usefulness. The little opera-glass with which Galileo first lifted the veil from the skies and awoke the thunders of the Vatican has blossomed out into the magnificent refractors of Cambridge, Chicago, and Washington. The little reflector with which Newton, by a happy mistake, expected to supplant the lens, has grown into the colossal telescopes of Herschel, Rosse, and the Melbourne observatory. The spasmodic, momentary action of Davy's batteries, sufficient, however, to inaugurate a new era in chemistry, has been superseded by constant currents, which grumble not at ten hours a day. After lighting up the forelands of a continent during the night they are fresh to work an ocean telegraph the next morning. With all my wonder at this mysterious instrument which serves so faithfully the cause of science and civilization, with renewed admiration of the microscope and the telescope, one of which transforms an invisible speck of matter into a universe and the other collects the immensity of the heavens into a little celestial globe upon the retina of the eye, I must pause for a moment to eulogize that simplest and most modest of scientific tools, the pendulum.

With the eye of science Galileo saw in the leaning Campanile at Pisa, not a freak of architecture, but the opportunity of experimenting on the laws of falling bodies: and, in the adjacent cathedral where others admired the marble pavement or the vaulted roof, the columns, statues, or paintings, his attention was caught

by the isochronous vibrations of the chandelier, which during the long centuries has never been absolutely at rest. When it is said that the pendulum has no rival as a standard of length except the metre, that it furnishes an exact measure of time, and that time is an indispensable element in the study of all motion, and also the most available means of obtaining longitude on the earth and right ascension in the heavens, a strong case has been made out for the practical and scientific usefulness of Galileo's discovery. During the long years of doubt in regard to the true figure of the earth, the pendulum maintained the cause of Newton in opposition to the erroneous reports of the geodesists, until Maupertuis, by a new measurement, flattened, as has been pithily said, the earth and the Cassinis at the same time. The shape, rotation, and density of the earth; the diminution of terrestrial gravity with an increase of distance from the centre; the local attractions of mountains, and secrets hidden below the surface of the planet, have been discovered or verified by the declarations of the pendulum: which, whether in motion or at rest, has never tired of serving science. And, in a wider sense, the pendulum has done for the electric and magnetic forces what, in its restricted meaning, it did for gravity. That which Borda failed of accomplishing in the measurement of arcs the pendulum realizes in its measurement of time: it multiplies its observations, eliminates its own errors, strikes its own average, and presents to science the perfect result. In 1851, a crowd of spectators was assembled in the Pantheon of Paris to witness the first performance by the pendulum of the new part prepared for it by Foucault: in which, obedient to its own inertia, and indifferent to the earth's rotation, it preserves the parallelism of its motion: an experiment startling though not wholly unanticipated, and which has made the circuit of the earth. The new contrivance of Zöllner promises to indicate changes in the direction of a force as accurately as the common pendulum measures intensity.

Let us now consider what the physicists of our own day, and their immediate predecessors, have added to their rich inheritance of instrumental means, remembering all the time that, however impressive from their novelty these additions may be, and however manifold their applications, they have only supplemented the experimental methods which have been described without supplanting them. For the most part, the later devices would be useless without the coöperation of the earlier ones.

An interesting event in the history of science, which must be known to many of you, has taken place during the current year. In 1824, Poggendorff began to edit the *Annalen der Chemie und der Physik*. Under his supervision 150 volumes have been issued, containing 8,850 distinct communications from 2,167 different authors, the 193 papers of H. Rose outnumbering those of any other contributor. The history of physical and chemical discovery during the last fifty years might be written out of the materials treasured up in this single journal. In recognition of the signal service which Poggendorff has hereby rendered to science, his friends assumed the editorship of one volume in 1874, which is called the Jubilee volume [Jubelband].

In 1826, Poggendorff described in volume vii. of his journal a device of his own invention for observing with exceeding nicety the movements of a magnetized bar. A mirror was attached to the bar and moved with it. From this mirror a beam of light was reflected into a theodolite. This was the origin of the happy thought of amplifying a trifling motion by making the finger of a long and delicate ray of light serve as a weightless pointer. A few years later, this idea was embodied by the mathematician, Gauss, in an instrument which he called the magnetometer. Since that time, it has been continually budding out in new applications, scientific and practical. I need only recall to your recollection the beautiful method of Lissajous for compounding the vibrations of tuning-forks, and tracing in golden lines the curves which are characteristic of different musical intervals and varied phases of vibration. A new chapter has been opened in mechanics for describing and explaining these strange and nameless curves; and, in acoustics, the ear has been dispossessed by the eye of what would seem to be its own by right divine, and it is no longer the best scientific judge of sounds. By new devices Koenig has translated time into space and made visible the individual vibrations of the invisible air; and, in numerous ways, the mechanism of sound is as real to the eye as the sensation is to the ear.

With a bare allusion to the fact that every message which passes over the cable telegraph is a tribute of indebtedness to the simple but comprehensive method of Poggendorff, I pass to two other cases of great difficulty and wide significance in which the same method has triumphed. I refer to the determination of the velocity of electricity and the velocity of light.

When Wheatstone devised and executed the ingenious experiment of producing three electrical sparks, not strictly at the same instant, but after the brief interval required by electricity to travel over one quarter of a mile of copper wire, and then of observing, not the sparks themselves, but their images, as seen in a mirror revolving with the prodigious velocity of 800 turns in a single second, and from the prolongation and relative displacement of these images deducing the velocity of electricity, the duration of the electrical light, and the duality in the direction of the transmitted disturbance, he delighted the brotherhood of science by the skill and boldness of his attempt and astonished it by the extravagance of his results. For twenty years no one ventured to repeat the difficult experiment. When at length it was tried by Feddersen, and more recently by our own associate, Rood, the values which they assigned to the duration of the electrical light, and which could not be challenged, made still the wonder grow. So far as this mode of experimenting concerns the velocity of electricity, Wheatstone stands alone: and his estimate of this velocity (the largest known velocity in the universe unless we count in the velocity of gravitation) has never been brought to a second trial. Indirectly, it has been tested by some of the operations conducted upon land and ocean lines of telegraph. When the local times of two places are compared by means of electro-magnetic signals, sent alternately in opposite directions, the difference of longitude and the transmission-time of electricity can be disentangled from one another, by the strategy of mathematics, and the most probable value computed for each. The velocity which has been calculated from these longitude-campaigns falls far below that credited to Wheatstone. The apparent discrepancy is explained by a misinterpretation of Wheatstone's experiment. An experiment which proves that electricity runs through one quarter of a mile of wire *at the rate* of 288,000 miles a second does not justify the inference that it would move over 288,000 miles in one second. Anomalous as the case may be, electricity has no velocity in the ordinary sense. The transmission time of the electrical disturbance is proportioned to the square of the distance to be travelled. Therefore, the velocity has no constant fixed value, but varies with the length of the journey. This law, which is deduced from the mathematical theory of Ohm, introduces order among the experiments where, otherwise, there would be chaos. It is not surprising that Wheat-

stone and the readers whom he addressed were misled by the original facts. Few men, who have rendered signal services to science, and who have finally reached the highest pinnacle of fame, have suffered more from poverty and neglect, and waited longer for a recognition of their merits, than the modest student of Nuremberg. The slender volume which will perpetuate his name was indeed published at Berlin in 1827, and antedates Wheatstone's experiment by seven years. But the book was treated with contempt by a minister of state, to whom Ohm presented a copy, at his university of Cologne, and was first brought to the notice of English readers in 1841, when an English translation of it was effected through the agency of the British Association, and the Copley medal was presented to Ohm by the Royal Society of London. As late as 1860, when the same work was rendered into French, the translator admits that the mathematical theory of Ohm on the galvanic circuit, the elements of which have since rapidly circulated in popular text-books, was almost unknown in France, that high seat of science. If the serene but steady light of mathematics had not been dimmed by the blaze of experimental successes, and the teachings of Ohm had been heeded sooner, the science of electricity would have been the gainer, and the men of science would have been saved the mortification of treating the electromagnetic telegraph as an impracticability.

When Wheatstone was a candidate to fill a vacancy among the corresponding members of the French Institute, it was objected that he had only made a brilliant experiment, but had not discovered a new principle. Arago came to his rescue and asserted that he had introduced a powerful and fertile method of experimentation which would be felt in other sciences besides electricity. The French physicist lost no time in devising means for making good these claims. If it could be proved experimentally that the velocity of light was greater in air than in water a capital fact in the contending theories of light would be settled forever. Arago planned the experiment and pressed its feasibility upon the Academy of Sciences with all the power and eloquence of his nature. At last he roused two younger physicists to undertake what his growing infirmities prevented him from doing with his own hands. The result declared in favor of undulations, and a fatal blow was dealt to the corpuscular theory of light which had vexed science since the days of Newton. If Fizeau and Foucault drew their in-

spiration from Arago, they owed their success to nothing except their own skill in devising and executing. Having tried the temper of their steel on this easier problem, they were ready for the grand attack, which was to measure the absolute velocity of light.

The instrumental arrangements of these two experimentalists agreed only in the part which each borrowed from Poggendorff: the details differed so widely as to give to whatever agreement might appear in their results the force of an irresistible argument for their accuracy. The velocity of light, as found by Fizeau in 1849 by the artificial eclipses which the teeth of his revolving wheel produced, exceeds by about six per cent. the velocity which Foucault obtained, in 1862, with the moving mirror. The arithmetical mean of the two values comes very close to the astronomer's estimate of the velocity of light. But this simple average is precluded unless it can be proved that the two experiments are entitled to equal weight. The internal evidence, expressed by what mathematicians call the probable error, manifested a decisive preference for Foucault's result, and it has met with general acceptance. The soundness of the scientific judgment in this case has been placed beyond all cavil by Cornu, who has recently repeated Fizeau's experiment, with additional precautions, and resolved the discord into a marvellous accord. Fizeau's experiment, in spite of the numerical defect, was hailed as one of the grandest triumphs of experimental skill. In 1856, he received the prize of 30,000 francs which the Emperor of the French had founded, to be given for the work or the discovery, which, in the opinion of the five academies of the Institute, had conferred the greatest honor and service upon the nation. Hitherto, it had been supposed that nothing short of an interstellar or an interplanetary space was a match for the enormous velocity of light. And yet one physicist, by using a distance of less than six miles, and another, without going outside of his laboratory, have discovered what astronomers had searched heaven and earth to find out.

By these capital experiments the science of optics has achieved its own independence. Let us see what they have done, at the same time, for astronomy. The sequences in the eclipses of Jupiter's moons are modified by the velocity of light. The aberration of starlight is a measure of the ratio between the velocity of light and the velocity of the earth. For nearly two centuries our knowledge of the velocity of light leaned upon one or the other

of these relations. If the velocity of light can be known from experiment, the problem may be reversed and the distance of the sun given to the astronomer. As soon at it appeared that Foucault's estimate of the velocity of light fell short of the astronomical valuation by about three per cent, it was certain that either the experiment was in error, or the received aberration was too small, or the reputed distance of the sun was too large. An error of three per cent. in the experiment or in the aberration was inadmissible. But it was conceivable that the distance of the sun should be at fault, even to this extent. The popular announcement that Foucault had picked a flaw in the astronomer's work was not correct. Astronomers had always known what those who pinned their scientific faith on text-books did not expect: that the problem of finding the sun's distance was an exceedingly delicate case, and that an ominous cloud of uncertainty hung over their wisest conclusions. Whenever it is possible to interrogate nature in more ways than one, science is not satisfied with a single answer, nor with all the answers unless they agree. The transit of Venus, the parallax of Mars, and the tables of the Moon, each can tell the sun's distance. But their testimony was contradictory, and neither one at all times repeated the same story. The question was, which to believe. Since 1824, when Encke published his exhaustive computations on the last transits of Venus, the distance which they assigned to the sun has been acquiesced in as the most probable. But the moon, as has been said, has always been a thorn in the sides of mathematicians. While practical and theoretical astronomers have been reducing its motions to stricter discipline, the suspicion has been steadily gaining strength in their minds that the distance adopted from the transits was too large. The effect of Foucault's experiment was to intensify the doubt. The case of the twin transits of the last century, thought to have been closed forever by Encke, has recently been opened again by the astronomer Stone. When Venus has nearly entered upon the sun, the moment of interior contact is preluded by the formation of a slender ligature (called the black drop) between the nearest parts of the two discs; caused, perhaps, by irradiation. One observer has recorded the time when this ligature began, another the time when it was broken. In working up the observations of the last transits, both classes were not combined indiscriminately. Mr. Stone has reëxamined the documents, classified differently the

materials, and extracted from them two new and independent values for the sun's parallax. The reconciliation which he has suddenly brought about between the experiments of Cornu and Foucault, the motions of the moon, and the transits of Venus, is as perfect as it is surprising. Nevertheless, the approaching transits of Venus, the earliest of which is close upon us, will be welcomed, if not as the only possible way of solving a hard problem, at least for the confirmation which is demanded by a solution already reached: for able astronomers have dissented from the interpretation put upon the records by Stone. The minds of observers have been prepared for what their eyes are to see, in December, 1874, by the experimental rehearsal of the black drop, and the photographer's box will arrest the planet in the very act.

The consequences of Foucault's experiment, substantiated as it may be by the best astronomical evidence, are as far reaching as the remotest stars and nebulæ. The sun's distance is the astronomer's metre, through which masses, diameters, and distances are proportioned out to planets, comets, and stars. If the sun's distance is cut down by three per cent., there must be a general contraction in all the physical constants of the universe. The earth only is immediately exempt from this liability. But if, as modern science teaches, the earth lives only by the triple radiation from the sun, then an earlier doom has been written for the earth also. Geology is no longer allowed to cut its garment from a past duration of unlimited extent. The numerical estimates of physical science, with a large margin of uncertainty, assign limits between which alone geology has free play. Whatever tends to reduce or enlarge those limits must be of interest to the geologist as well as to the astronomer.

This is the brilliant career, in electricity, optics, astronomy, and geology, of the little mirror, cradled in the laboratory of Poggendorff, and which has not yet seen its fiftieth birthday.

In making this exhibit of the instrumental appliances of modern physics, I will simply name the polariscope, the stereoscope, and the instruments in photography, and hurry on to the spectroscope.

The steps by which the spectroscope has attained its preëminent rank among the instruments of the physicist and the astronomer were taken at long intervals. A whole century intervened between Newton's experiments with the prism and Wollaston's improvement. The substitution of a long and narrow slit for the round

hole in the window shutter was enough to reveal the presence of the two boldest dark lines in the solar spectrum. Wollaston stood on the threshold of a rich development in science, but neither he nor his compeers were ready for it, and what he saw, novel as it was, attracted little attention. Spectrum analysis, in relation to light itself, began when Frauenhofer published, in 1817, in the memoirs of the Bavarian Academy, an account of his experiments on the direct and reflected rays of the sun, on starlight, and various artificial sources of light: dispersing the rays by prisms of fine Munich glass and then receiving them into a theodolite. Frauenhofer repeated some of his experiments in the presence of the younger Herschel, but for many years he had the field wholly to himself. A paper by Herschel on the colors of artificial flames acquires a new interest from what has been done more recently. Between 1830 and 1860, numerous physicists, among whom are the well known names of Brewster, Miller, Wheatstone, Powell, Stokes, Gladstone, Becquerel, Masson, Van der Willigen, Plücker, and Angström, were at work upon the facts connected with the emission of light by incandescent bodies and its absorption by gases and vapors. As early as 1830, Simms had placed a lens in front of the prism, with the slit in the focus, and another lens behind the prism to form an image of the slit.

The first hint of that pregnant fact, the reversal of the bright spectrum bands of flames, came from Foucault in 1849. His experiment was repeated at Paris, in 1850, in the presence of Sir William Thomson. It was reserved for a young physicist of Heidelberg, who was not born until seven years after Frauenhofer laid the foundations, to place the keystone upon the structure on which many hands had labored: by demonstrating, in 1860, the law which is the theoretical basis of the chemistry of the heavens. Kirchhoff, with admirable frankness, is careful to say that this law had been anticipated by others, especially by Angström and Balfour Stewart, although it had not been sharply stated or severely proved. It is a singular fact that the mechanical explanation of the law, as it has been expounded by Kirchhoff, Angström, and Stokes, was partially enunciated one hundred years ago by the mathematician, Euler, when he said that every substance absorbs light of the special wave-length which corresponds to the vibration of its smallest particles. The 11th of July, 1861, will be ever memorable in the history of science as being the day

on which Magnus read, before the Berlin Academy, Kirchhoff's memoir on the chemical constitution of the sun's atmosphere, and the existence in it of familiar substances found upon the earth. Speedily, spectroscopes were multiplied, modified, and improved, and became indispensable auxiliaries in the workshop, the laboratory, and the observatory. It is not necessary to enlarge upon what this instrument has done for common chemistry, in hunting out the minutest traces of common substances and detecting new ones. The physician, the physiologist, the zoölogist, the botanist, and the technologist have shared with the chemist and the physicist the services of this powerful analyst. But it is the highest prerogative of the spectroscope to be able to make a chemical analysis of celestial bodies, upon the single condition that they give to it their light. Polarization can only say whether any portion of this light is reflected. The motions which the telescope uncovers may decide in favor of a central attraction, but it is silent as to the intensity of this attraction unless the moving body belongs to the solar system. The universality of a gravitation may be proved, but not the universality of the very gravitation which pervades our own system; except by an argument from analogy. We see that one star differs from another star in glory. But what the other differences or resemblances are we know not, without the spectroscope. Henceforth astronomy possesses a new instrument of discovery, and also a new tribunal to which all speculations about the sun and the stars, the aurora and the zodiacal light, the meteors and the comets, must be brought and by which they must be judged.

I leave it to the naturalists to assign a value to the alleged anticipations of Darwin by the geometer Maupertuis, who was said to have died just before he was going to make monkeys talk. The whims and conceit of Lord Monboddo are not worthy of notice. Lamarck began life as a soldier: was a meteorologist as far and as long as Napoleon would allow him to be: perhaps he was a botanist from choice, but he was made a zoölogist, in spite of himself, by the revolutionary Convention. He was as brave in science as in war; but he expected to *create* it, by a simple effort of thought. Having demolished the modern chemistry, he turned his iconoclastic zeal into natural history. His philosophy of zoölogy was published a few years after the cosmogony of Laplace; in which the mathematician broaches the theory of evo-

lution as a mechanical doctrine, capable of explaining certain characteristics of the solar system, about which the law of gravitation is silent. Whoever reads the stately chapters of Laplace, on the stability of the planets and the safeguards of the comets, will easily recognize expressions which are the mechanical equivalents of the principles of natural selection and the survival of the fittest. The elder Herschel hazarded the speculation, that the clusters of stars and the nebulæ which his devouring telescope had picked up, by hundreds, on the verge of the visible heavens, were genuine suns assembled under the organizing power of gravitation; and that the varieties in size, shape, and texture, were produced by differences of age and distance. The imagination of Herschel and other astronomers has taken a loftier flight. To them many of the nebulæ are not clusters of stars, but unborn solar systems, waiting for that consolidation by which planets are evolved and a central sun is formed, and destined thus to repeat the cosmogony of the home system. Comte claims that he has raised the nebular hypothesis to the rank of positive science. He supposes the stupendous engiuery of evolution to be reversed. He follows, with his mathematics, the expanding sun backwards into chaos, until it has absorbed into its bosom even the first born among the planets, and finds, at every stage, numerical confirmation of what Laplace threw out as a plausible conjecture. As Mr. Mill and other writers of note have accepted this authority, it should be understood that Comte has never published the data or the process of his computations. By whatever other inspiration he arrived at his conclusion, he was not brought to it by his mathematics. He has said all that is necessary to show that he ignored all the difficulties of the problem, and dodged the only solution that could give satisfaction. The cosmogony of Laplace, with all its fascination, must be excluded from exact mechanics and remanded back to its original place in natural history, by the side of the more general nebular hypothesis of Herschel. All other cosmogonies which poetry or science have invented are childish in comparison with this: and no one would desire to banish it from science altogether, until it is disproved or displaced by something better. Instead of *deciding*, it must *share* the fate of the all-embracing cosmical speculation of Halley. How uncertain that fate is we may be taught by the frequency with which the preponderance of evidence has shifted from one

side to the other, during the last fifty years. The irresolvability of many of the nebulæ, by powerful telescopes, led Herschel to espouse the cause of a diffuse primeval matter, out of which worlds were fashioned. No wonder that, in particular cases, the negative evidence was sometimes turned into positive evidence on the other side, by improvements in telescopes. Although every nebula which deserted from the nebular hypothesis strengthened the suspicion that the remaining irresolvability was purely optical, a sufficient amount of negative evidence would probably have always existed to create more than a doubt in the minds of many astronomers. On the discovery of spectrum analysis, observers rallied around it, in the hope of finding an escape from the dilemma: and this new hope has not been disappointed. The continuous spectra of some nebulæ prove them to be suns, enveloped in more or less of atmosphere. The broken spectra of other nebulæ show that they are in the condition of an incandescent gas. The classification which the spectroscope makes of the nebulæ corresponds so well with their telescopic appearance as to justify the confidence which one class of astronomers had in their way of deciding on the truth of the nebular hypothesis. While the spectroscope has manifested varieties of material, color, temperature, and consolidation in nebulæ and stars, both single and composite, beyond anything which the perfected telescope could ever have revealed, it has at the same time found enough of earth in all of them to make man feel at home any where in the visible universe. The fact that certain well-known substances on this planet pass current everywhere in nature leads irresistibly to the conclusion that all the specimens came originally from the same mint. It is the legitimate office of science to reduce the more complex to the simple: to explain, if possible, the existing state of matter by an anterior state. The nebular hypothesis, which attempts to do this, no longer starts from a conjecture but a reality: *viz.*, the existence of diffused incandescent vapor; and science will hold on to it, until a better theory of mechanical development is found.

An interesting question, which has waited thousands of years even to be asked, and may wait still longer for an all-sufficient answer, relates to the motion of what were once called the fixed stars. If numbers count for anything, this is the grandest problem which can be presented to the mind of the astronomer. The

argument from probabilities, which reposes on a substantial mathematical foundation, is loud in affirming some kind of motion, and repudiates the notion of absolute rest. We must place the stars outside the pale of science, and where no process of reasoning can reach them, or we must suppose that they subscribe to the universal law of all matter which we know, and exert attractive or repulsive forces upon each other. There may be one solitary body, or more probably an ideal point of space, the centre of gravity of the material universe, around which there is equilibrium: but everywhere else there must be motion. Though distance may reduce the effect of each one of the forces to a minimum, in the aggregate their influence will not be insignificant. The sun must share the common lot of the stars unless we repeat the folly of ancestral science, at which we now smile, and transfer the throne of the heaven of matter from the earth to the centre of our own little system. If the sun moves, a new order of parallactic motion springs up in sidereal astronomy. The process of elimination requires the mathematician to calculate the direction and velocity of the motion of the sun which will leave behind it the smallest unexplained residuum: and this remainder is the motion of the stars themselves. The delicacy of the problem lies in the minuteness of the quantities to be observed and in the assumptions which must be made in regard to the distances of the stars; only a few of which have been positively computed from parallax. However, a result has been reached, highly probable in the sun's case, but which can be converted into absolute values for other stars only so fast as their individual distances are discovered. Here again physics and chemistry, with the spectroscope in hand, have come to the aid of astronomy and geometry. Should it appear that the conclusions from spectrum analysis must be questioned, the attempt was brave, and even a defeat would be honorable.

In 1675, a Danish astronomer observed the novel fact that the frequency in the eclipses of Jupiter's satellites fluctuated with the motion of the planet to or from the earth. He hit upon a happy explanation, viz: that the swift light takes more or less time to telegraph the astronomical news across the omnipresent lines of force. This early observation is the avant-courier of a host of others which have slowly followed in close array. That of a blind musician comes next. He noticed, in 1835, that the

pitch of a steam-whistle, on the Lowell Railroad, fell suddenly as the locomotive passed him. Unfortunately, Munroe's observation was never published, although he sought and found an explanation of what was then a strange fact. In this case, the whistle sends the message, the waves of sound transmit it, and the ear is the register: but the changing distance modifies the time. In 1842, Doppler of Prague was led, by theoretical considerations, to formulate the proposition, now known in science as Doppler's principle: that the color of light and the pitch of sound, as they tell upon the senses, are changed by the relative velocity of the observer and the origin of the disturbance. In 1845, Buy Ballot made experiments upon the railroads in the Netherlands, and Scott Russell repeated them on English railroads, which confirmed the theory in the case of sound. In the application of the theory to color, few astronomers will be willing to follow Doppler in all his extravagancies.

If it be true, theoretically, that the relative velocity of light, the wave-length of transmission, and the period of oscillation in the ether, are altered by the relative motion of the observer and the place from which the undulation starts, it is obvious that all other velocities have but a small chance in competition with the velocity of light, and that slight changes of color, if physically real as Doppler supposed, would fail of being recognized even by the eye of a painter. To interpose the spectroscope, and observe the change of refrangibility by the displacement of the sharp lines of the spectrum, was a lucky escape from this embarrassment. After Huggins had tried his hand at this new method, with a small telescope, upon the brightest of all the stars, he was supplied by the Royal Society of London with a larger instrument to pursue the investigation. The results of his spectroscopic inquiry into the motions of many stars have been published. Where these results have conflicted with the foregone conclusions of astronomy, Huggins has not hesitated to arraign the accuracy of astronomical data and methods. I have freely admitted the delicacy and difficulty of the geometrical process. The spectroscopic analysis, when applied to the same problem, walks upon slippery ground and must take heed lest it also fall. The alleged displacement is a nice quantity, and instrumental sources of error have been pointed out which may explain away the whole of it. I lay no stress upon the large difference between Vogel and

Huggins in the *quantity* of motion which spectrum analysis ascribes to Sirius, inasmuch as the direction of the motion is the same. We do not yet know all the elements which the earth contains. The spectroscope has already added four to the number. There is reason to think that the stars, though having some substances in common with the earth and sun, are not without their peculiarities. The lines in the stellar spectra may be out of position, not because they are the displaced lines of sodium, magnesium, and hydrogen, but in consequence of novelties in the gaseous atmospheres of the stars. Still, there will be a presumption, perhaps a probability, in favor of Huggins' deduction, if it rests on a sound basis of theory. If there is any weakness in the physical and mathematical foundation of his argument, gratifying as it is to the imagination and the aspirations of science, the whole superstructure must fall.

I am thus suddenly brought face to face, with the second head of my subject: the mathematical and philosophical state of the physical sciences.

The luminiferous ether and the undulatory theory of light have always troubled what is supposed to be the imperturbable character of the mathematics. The proof of a theory is indisputable when it can predict consequences, and call successfully upon the observer to fulfil its prophecies. It is the boast of astronomers that the law of gravitation thus vindicates itself. The undulatory theory of light has shown a wonderful facility of adaptation to each new exigency in optics, and has opened the eye of observation to see what might never have been discovered without the promptings of theory. But this doctrine, and that of gravitation also, have more than once been arrested in their swift march and obliged to show their credentials. After Fresnel and Young had secured a firm foothold for Huyghens' theory of light in mechanics and experiment, questions arose which have perplexed, if not baffled, the best mathematical skill. How is the ether affected by the gross matter which it invests and permeates? Does it move when they move? If not, does the relative motion between the ether and other matter change the length of the undulation or the time of oscillation? These queries cannot be satisfactorily answered by analogy, for analogy is in some respects wanting between the ether and any other substance. Astronomy says that aberration cannot be explained unless the ether is at rest. Optics

replies that refraction cannot be explained unless the ether moves. Fresnel produced a reconciliation by a compromise. The ether moves with a *fractional* velocity large enough to satisfy refraction, but too small to disturb sensibly the astronomer's aberration. In 1814, Arago reported to Fresnel that he found no sensible difference in the prismatic refraction of light, whether the earth was moving with full speed towards a star or in the opposite direction, and asked for an explanation. Fresnel submitted the question to mathematical analysis, and demonstrated, that whatever change was produced by the motion of the prism in the relative velocity of light, the wave-length in the prism, and the refraction, was compensated by the physiological aberration when the rays emerged. Very recently, Ketteler of Bonn has gone over the whole ground again with great care, studying not only Arago's case but the general one, in which the direction of the light made any angle with the motion of the earth: and he proves that the light will always enter the eye in the same apparent direction as it would have done if the earth were at rest. The mathematical and physical view taken of this subject by Fresnel, has been under discussion for sixty years, and forty eminent physicists and mathematicians might be enumerated who have taken part in it. Fresnel's explanation has encountered difficulties and objections. Still, it is consistent not only with Arago's negative result but with the experiments on diffraction by Fizeau and Babinet, and the preponderance of mathematical evidence is on that side. Mr. Huggins runs counter to the general drift of physical and algebraical testimony (although he appears to be sustained by the high authority of Maxwell), when he attributes some displacement of the spectrum lines to the motion of the earth, and qualifies the observed displacement on that account. The number of stars which Huggins has observed is insufficient for any sweeping generalization. And yet he seems inclined to explain the revelations of his spectroscope, not by the motion of the stars, but by that of the solar system: because those stars which are in the neighborhood of the place in which astronomers have put the solar apex are moving, apparently, towards the earth, while those in the opposite part of the sky recede. If it be true that the earth's annual motion produces no displacement in the spectrum, then the motion of the solar system produces none. Or, waiving this objection, if the correct explanation has been given by Huggins, astronomers

have failed, by their geometrical method, of rising to the full magnitude of the sun's motion. The discrepancy appears to awaken no distrust in Mr. Huggins' mind as to the delicacy of the spectrum analysis or the mathematical basis of his reasoning. On the contrary, he would remove the discrepancy by throwing discredit on the estimate of star-distances made independently by Struve and Argelander from different lines of thought.

Next we ask, if it is certain that even the motion of the luminary will change the true wave-length, the period of oscillation, and the refrangibility, of the light which issues from it. The commonly received opinion on this subject has not been allowed to pass unchallenged. It is fortified by more than one analogy: but it is said that comparison is not always a reason. It is not denied that, when the sonorous body is approaching, the sound waves are shortened, the number of impulses on the ear by the condensed air is increased, and the pitch of the sound is raised. Possibly, the color of light would follow the same law; but there is no experiment to prove it, and very little analogy exists between the eye and the ear. There is no analogy, whatever, between the subjective sensation by either organ and the physical action of the prism. The questions at issue are these:—Does refraction depend upon the absolute or the relative velocity of light; are the time of oscillation of the particles of ether and the normal wave-length, corresponding to it, changed by any motion of translation in the origin; or is the conservation of these elements an essential attribute of the luminiferous medium. It has been said that Doppler reasoned as if the corpuscular theory of light were true, and then expressed himself in the language of undulations. Evidently, there is an obscurity in the minds of many physicists, and an uncertainty in all, when they reason upon the mechanical constitution of the ether, and the fundamental laws of light. The mathematical theory is not so clear as to be able to dispense with the illumination of experiment. Within the present year, Van der Willigen has published a long and well considered memoir on the theoretical fallacies which vitiate the whole of Huggins' argument for the motion of the stars and nebulæ. His analysis proves that the motion of the luminary will not interfere with the time of oscillation and the wave-length, provided that the origin of the disturbance is not a mathematical point but a vibrating molecule, and that the sphere of action of this molecule upon surrounding mole-

cules is large enough to keep them under its influence during ten or a hundred vibrations, before it is withdrawn by the motion of translation. If this theoretical exposition of the subject should be generally adopted by mathematicians, the spectroscopic observations on the supposed motion of the stars must receive another interpretation. On the other hand, if a luminary is selected which is known to move, independently of spectroscopic observations, and the displacement of the spectrum lines accords with this motion, it will be time to reconsider the mathematical theory, and make our conceptions of the ether conform to the experiment. The spectroscopic observation of Angström on an oblique electric spark does not favor Huggins' views. Secchi testifies to opposite displacements when he examined, with a direct vision spectroscope, the two edges of the sun's equator, one of which was rotating towards him and the other from him, and Vogel has repeated the observation with a reversion-spectroscope. This would have the force of a crucial experiment were it not that an equal displacement was seen on other parallels of latitude, and that the bright bands of the chromosphere were moved but not the dark lines of the solar atmosphere.

When Voltaire visited England in 1727 he saw at the universities the effect of Newton's revolutionary ideas in astronomy. The mechanism of gravitation had exiled the fanciful vortices of Descartes, which were still circulating on the continent. So he wrote: "A Frenchman who comes to London finds many changes in philosophy as in other things: he left the world full, he finds it empty." The same comparison might be made now, not so much between nationalities as between successive stages of scientific development. At the beginning of this century the universe was as empty as an exhausted receiver: now it has filled up again. Nature's abhorrence of a vacuum has been resuscitated, though for other reasons than those which satisfied the Aristotelians. It is the mathematicians and not the metaphysicians who are now discussing the relative merits of the *plenum* and the *vacuum*. Newton in his third letter to Bentley wrote in this wise:—"That gravity should be innate, inherent and essential to matter, so that one body may act upon another at a distance, through a *vacuum*, without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity, that I believe no man, who has in philo-

sophical matters a competent faculty of thinking, can ever fall into it." Roger Cotes, who was Newton's successor in the chair of mathematics and natural philosophy at Cambridge, was only four years old when the first edition of the *Principia* was issued, and Newton outlived him by ten years. The venerable teacher pronounced upon the young mathematician, his pupil, these few but comprehensive words of eulogy: "If Cotes had lived, we should have known something." The view taken of gravitation by Cotes was not the same as that held by his master. He advocated the proposition that action at a distance must be accepted as one of the primary qualities of matter, admitting of no farther analysis. It was objected by Hobbes and other metaphysicians, that it was inconceivable that a body should act where it was not. All our knowledge of mechanical forces is derived from the conscious effort we ourselves make in producing motion. As this motion employs the machinery of contact, the force of gravitation is wholly outside of all our experience. The advocates of action at a distance reply, that there is no real contact in any case, that the difficulty is the same with the distance of molecules as that of planets, that the mathematics are neither long-sighted nor short-sighted, and that an explanation which suits other forces is good enough for gravitation.

Comte extricated himself from this embarrassment by excluding causes altogether from his positive philosophy. He rejects the word attraction as implying a false analogy, inconsistent with Newton's law of distance. He substitutes the word gravitation, but only as a blind expression by which the facts are generalized. According to Comte's philosophy, the laws of Newton are on an equality with the laws of Kepler, only they are more comprehensive, and the glory of Kepler has the same stamp as that of Newton. Hegel, the eminent German metaphysician, must have looked at the subject in the same light when he wrote these words: — "Kepler discovered the laws of free motion; a discovery of immortal glory. It has since been the fashion to say that Newton first found out the truth of these rules. It has seldom happened that the honor of the first discoverer has been more unjustly transferred to another." Schelling goes farther in the same direction: he degrades the Newtonian law of attraction into an empirical fact, and exalts the laws of Kepler into necessary results of our ideas.

Meanwhile, the Newtonian theory of attraction, under the skilful generalship of the geometers, went forth on its triumphal march through space, conquering great and small, far and near, until its empire became as universal as its name. The whirlpools of Descartes offered but a feeble resistance, and were finally dashed to pieces by the artillery of the parabolic comets; and the rubbish of this fanciful mechanism was cleaned out as completely as the cumbrous epicycles of Ptolemy had been dismantled by Copernicus and Kepler. The mathematicians certified that the solar system was protected against the inroads of comets, and the border warfare of one planet upon another, and that its stability was secure in the hands of gravitation, if only space should be kept open, and the dust and cobwebs which Newton had swept from the skies should not reappear. Prophetic eyes contemplated the possibility of an untimely end to the revolution of planets, if their ever expanding atmospheres should rush in to fill the room vacated by the maelstroms of Descartes. When it was stated that the absence of infinite divisibility in matter, or the coldness of space, would place a limit upon expansion, and, at the worst, that the medium would be too attenuated to produce a sensible check in the headway of planets, and when, in more recent times, even Encke's comet showed but the slightest symptoms of mechanical decay, it was believed that the motion was, in a practical, if not in a mathematical sense, perpetual. Thus it was that the splendors of analysis dimmed the eyes of science to the intrinsic difficulties of Newton's theory, and familiarity with the language of attraction concealed the mystery that was lurking beneath it. A long experience in the treatment of gravitation had supplied mathematicians with a fund of methods and formulas suited to similar cases. As soon as electricity, magnetism and electro-magnetism took form, they also were fitted out with a garment of attractive and repulsive forces acting at a distance: and the theories of Cavendish, Poisson, Aepinus and Ampere, endorsed as they were by such names as Laplace, Plana, Liouville and Green, met with general acceptance.

The seeds, which were destined to take root in a later generation, and disturb if not dislodge the prevalent interpretation of the force of gravitation, were sown by a contemporary of Newton. They found no congenial soil in which they could germinate and fructify until the early part of the present century. At the

present moment, we find the luminiferous ether in quiet and undivided possession of the field from which the grosser material of ancient systems had been banished. The *plenum* reigns everywhere; the vacuum is nowhere. Even the corpuscular theory of light, as it came from the hands of its founder, required the reinforcement of an ether. Electricity and magnetism, on a smaller scale, applied similar machinery. If there was a fundamental objection to the conception of forces acting at a distance, certainly the bridge was already built by which the difficulty could be surmounted. The turning-point between the old physics and the new physics was reached in 1837, when Faraday published his experiments on the specific inductive capacity of substances. This discovery was revolutionary in its character, but it made no great stir in science at the time. The world did not awake to its full significance until the perplexing problem of ocean telegraphs converted it from a theoretical proposition into a practical reality, and forced it on the attention of electricians. The eminent scientific advisers of the cable companies were the first to do justice to Faraday. This is one of the many returns made to theoretical electricity for the support it gave to the most magnificent commercial enterprise.

The discovery of diamagnetism furnished another argument in favor of the new interpretation of physical action. What that new interpretation was, is well described by Maxwell. "Faraday, in his mind's eye, saw lines of force traversing all space, where the mathematicians saw centres of force attracting at a distance; Faraday saw a medium where they saw nothing but distance; Faraday sought the seat of the phenomena in real actions going on in the medium, they were satisfied that they had found it in a *power* of action at a distance impressed on the electric fluids." The physical statement waited only for the coming of the mathematicians who could translate it into the language of analysis, and prove that it had as precise a numerical consistency as the old view with all the facts of observation. A paper published by Sir William Thomson, when he was an undergraduate at the university of Cambridge, pointed the way. Prof. Maxwell, in his masterly work on electricity and magnetism, which appeared in 1873, has built a monument to Faraday, and unconsciously to himself also, out of the strongest mathematics. For forty years mathematicians and physicists had labored to associate the laws

of electrostatics and electrodynamics under some more general expression. An early attempt was made by Gauss in 1835, but his process was published, for the first time, in the recent complete edition of his works. Maxwell objects to the formula of Gauss because it violates the law of the conservation of energy. Weber's method was made known in 1846; but it has not escaped the criticism of Helmholtz. It represents faithfully the laws of Ampere and the facts of induction, and led Weber to an absolute measurement of the electrostatic and electromagnetic units. The ratio of these units, according to the formulas, is a velocity; and experiment shows that this velocity is equal to the velocity of light. As Weber's theory starts with the conception of action at a distance, without any mediation, the effect would be instantaneous, and we are at a loss to discover the physical meaning which he attaches to his velocity. Gauss abandoned his researches in electromagnetism because he could not satisfy his mind in regard to the propagation of its influence in time. Other mathematicians have worked for a solution, but have lost themselves in a cloud of mathematical abstraction. The two theories of light have exhausted all imaginable ways in which force can be gradually transmitted without increase or loss of energy. Maxwell cut the Gordian knot when he selected the luminiferous ether itself as the arena on which to marshal the electromagnetic forces under the symbols of his mathematics, and made light a variety of electromagnetic action. His analysis gave a velocity essentially the same as that of Weber, with the advantage of being a physical reality and not a mere ratio. Of the two volumes of Mr. Maxwell, freighted with the richest and heaviest cargo, the reviewer says: "Their author has, as it were, flown at everything: and, with immense spread of wing and power of beak, he has hunted down his victims in all quarters, and from each has extracted something new and interesting for the intellectual nourishment of his readers." Clear physical views must precede the application of mathematics to any subject. Maxwell and Thomson are liberal in their acknowledgements to Faraday. Mr. Thomson says: "Faraday, without mathematics, divined the result of the mathematical investigation; and, what has proved of infinite value to the mathematicians themselves, he has given them an articulate language in which to express their results. Indeed, the whole language of the *magnetic field* and *lines of force* is Faraday's. It must be said

for the mathematicians that they greedily accepted it, and have ever since been most zealous in using it to the best advantage."

It is not expected that the new views of physics will be generally accepted without vigorous opposition. A large amount of intellectual capital has been honestly invested in the fortunes of the other side. The change is recommended by powerful physical arguments, and it disenthralls the theories of science from many metaphysical difficulties which weigh heavily on some minds. On the other hand, the style of mathematics which the innovation introduces is novel and complex; and good mathematicians may find it necessary to go to school again before they can read and understand the strange analysis. It is feared that with many who are not easily deflected from the old ruts, the intricacies of the new mathematics will outweigh the superiority of the new physics.

The old question, in regard to the nature of gravitation, was never settled: it was simply dropped. Now it is revived with as much earnestness as ever, and with more intelligence. Astronomy cast in its own mould the original theories of electrical and magnetic action. The revolution in electricity and magnetism must necessarily react upon astronomy. It was proved by Laplace, from data which would now, probably, require a numerical correction, that the velocity of the force of gravitation could not be less than eight million times the velocity of light; in fact, that it was infinite. Those who believe in action at a distance cannot properly speak of the transmission of gravitation. Force can be transmitted only by matter: either with it or through it. According to their view, action at a distance *is* the force, and it admits of no other illustration, explanation, or analysis. It is not surprising that Faraday and others, who had lost their faith in action at short distances, should have been completely staggered by the ordinary interpretation of the law of gravitation, and that they declared the clause which asserted that the force diminished with the square of the distance to be a violation of the principle of the conservation of force.

Must we then content ourselves with the naked facts of gravitation, as Comte did, or is it possible to resolve them into a mode of action in harmony with our general experience, and which does not shock our conceptions of matter and force? In 1798, Count Rumford wrote thus: "Nobody surely, in his sober senses, has ever pretended to understand the mechanism of gravitation."

Probably Rumford had never seen the paper of Le Sage, published by the Berlin Academy in 1782, in which he expounded his mechanical theory of gravitation, to which he had devoted sixty-three years of his life. In a posthumous work, printed in 1818, Le Sage has developed his views more fully. He supposed that bodies were pressed towards one another by the everlasting pelting of ultramundane atoms, inward bound from the immensity of space beyond, the faces of the bodies which looked towards each other being mutually screened from this bombardment. It was objected to this hypothesis, which introduced Lucretius into the society of Newton and his followers, that the collision of atoms with atoms, and with planets, would cause a secular diminution in the force of gravity. Le Sage admitted the fact. But as no one knew that the solar system was eternal, the objection was not fatal. As the necessity for giving a mechanical account of gravitation was not generally felt at the time, the theory of Le Sage fell into oblivion. In 1873, Sir William Thomson resuscitated and republished it. He has fitted it out in a fashionable dress, made out of elastic molecules instead of hard atoms, and has satisfied himself that it is consistent with modern thermo-dynamics and a perennial gravitation.

Let us now look in a wholly different quarter for the mechanical origin of gravitation. In 1870, Prof. Guthrie gave an account of a novel experiment, viz:—the attraction of a light body by a tuning-fork when it was set in vibration. Thomson repeated the experiment upon a suspended eggshell and attracted it by a simple wave of the hand. Thomson remarks "that what gave the great charm to these investigations, for Mr. Guthrie himself, and no doubt also for many of those who heard his expositions and saw his experiments, was, that the results belong to a class of phenomena to which we may hopefully look for discovering the mechanism of magnetic force, and possibly also the mechanism by which the forces of electricity and gravity are transmitted." By a delicate mathematical analysis, Thomson arrives at the theorem that the "average pressure at any point of an incompressible, frictionless fluid, originally at rest, but set in motion and kept in motion by solids, moving to and fro, or whirling round in any manner, through a finite space of it" would explain the attractions just described. Moreover, he is persuaded by other effects besides those of light, that, in the interplanetary spaces and in the best

artificial vacuum, the medium which remains has "perfectly decided mechanical qualities, and, among others, that of being able to transmit mechanical energy, in enormous quantities:" and he cherishes the hope that his mathematical theorems on abstract hydrokinetics are of some interest in physics as illustrating the great question of the eighteenth and nineteenth centuries:—Is action at a distance a reality, or is gravitation to be explained, as we now believe magnetic and electric forces must be, by action of intervening matter?

In 1869 and 1873, Prof. Challis of Cambridge, England, published two works on the Principles of Mathematical Physics. They embody the mature reflexions of a mathematical physicist at the advanced age of three score years and ten. Challis believes that there is sufficient evidence for the existence of ether and atoms as physical realities. He then proceeds to say:—"The fundamental and only admissible idea of *force* is that of *pressure*, exerted either actively by the ether against the surface of the atoms, or as reaction of the atoms on the ether by resistance to that pressure. The principle of deriving fundamental physical conceptions from the indications of the senses does not admit of regarding *gravity*, or any other force varying with distance, as an essential quality of matter, because, according to that principle, we must, in seeking for the simplest idea of physical force, have regard to the sense of *touch*. Now, by this sense, we obtain a perception of force as pressure, distinct and unique, and not involving the variable element of distance, which enters into the perception of force as derived from the sense of sight alone. Thus, on the ground of simplicity as well as of distinct perceptibly, the fundamental idea of force is pressure." As all other matter is passive except when acted upon by the ether, the ether itself, in its quiescent state, must have uniform density. It must be coextensive with the vast regions in which material force is displayed. Challis had prepared himself for the elucidation and defence of his dynamical theory by a profound study of the laws of motion in elastic fluids. From the mathematical forms in which he has expressed these laws he has attempted to derive the principal experimental results in light, heat, gravitation, electricity, and magnetism. Some may think that Mr. Challis has done nothing but clothe his theory in the cast off garments of an obsolete philosophy. If its dress is old, it walks upon new legs. The

interplay between ether and atoms is now brought on to the stage, not as a speculation supported by metaphysical and theological arguments, but as a physical reality with mathematical supports. I should do great injustice to this author if I left the impression that he himself claimed to have covered the whole ground of his system by proof. Mathematical difficulties prevented him from reaching a numerical value for the resultant action of a wave of ether upon the atom. What he has written is the guidepost, pointing the direction in which science is next to travel: but the end of the journey is yet a great way off. The repeated protests of Mr. Challis against the popular physics of the day, and his bold proclamation of the native, independent motion of the ether, have aroused criticism. What prevents the free ether, asks the late Sir John Herschel, from expanding into infinite space? Mr. Challis replies that we know nothing about infinite space or what happens there, but the existence of the ether, where our experience can follow it, is a physical reality. The source of the motion which the ether acquires is not the sun: for the most efficient cause of solar radiation is gravitation and condensation. Our author avoids the vicious circle of making gravitation, first the reason and afterwards the consequence of the motion of the ether. He says: "it follows that the sun's heat, and the heat of masses in general, are stable quantities, oscillating, it may be, like the planetary motions, about *mean* values, but never permanently changing, so long as the Upholder of the universe conserves the force of the ether and the qualities of the atoms. There is no law of destructibility: but the same Will that conserves can in a moment destroy." The following remarks upon this theory deserve our attention. "The explanation of any action between distant bodies by means of a clearly conceivable process, going on in the intervening medium, is an achievement of the highest scientific value. Of all such actions that of gravitation is the most universal and the most mysterious. Whatever theory of the constitution of bodies holds out a prospect of the ultimate explanation of the process by which gravitation is effected, men of science will be found ready to devote the whole remainder of their lives to the development of that theory."

The hypotheses of Challis and Le Sage have one thing in common; the motion of the ether and the driving storm of atoms must come from outside the world of stars. "On either theory,

the universe is not even temporarily automatic, but must be fed from moment to moment by an agency external to itself." Our science is not a finality. The material order which we are said to know makes heavy drafts upon an older or remoter one, and that again upon a third. The world, as science looks at it, is not self-sustaining. We may abandon the hope of explaining gravitation, and make attraction itself the primordial cause. Our refuge then is in the sun. When we qualify the conservation of energy by the dissipation of energy, the last of which is as much an induction of science as the first, the material fabric which we have constructed still demands outward support. Thomson calculates that, within the historical period, the sun has emitted hundreds of times as much mechanical energy as is contained in the united motions of all the planets. This energy, he says, is dissipated more and more widely through endless space, and never has been, probably never can be, restored to the sun, without acts as much beyond the scope of human intelligence as a creation or annihilation of energy, or of matter itself, would be.

From the earliest dawn of intellectual life, a general theory of the constitution of matter has been a fruitful subject of debate, and human science and philosophy have ever been dashing their heads against the intractable atoms. The eagerness of the discussion was the greater, the more hopeless the solution. For every man who set up an hypothesis upon the subject there were half a dozen others to knock it down; until at last speculation, which bore no fruit, was suspended. A lingering interest still hung around the question, whether matter was not infinitely divisible, and the atomic philosophers were not chasing a chimera. From every new decision on this single point there was an appeal, and the foothold which the atoms had secured in chemistry was gradually subsiding. Of a sudden, the atomic theory has gained a new lease of life. But the hero of the new drama is not the atom but the molecule. In all the physical sciences, including astronomy, the war has been carried home to the molecules: and the intellectual victories of this and the next generation will be on this narrow field. From the outlying provinces of physics; from the sun, the stars and the nebulæ; from the comets and meteors; from the zodiacal light and the aurora; from the exquisitely tempered and mysterious ether; the forces of nature have been moving in converging lines to this common battle ground, and some shouts of

victory have already been heard. In the long and memorable controversy between Newton and Leibnitz, and their adherents, as to the true measure of force, it was charged against the Newtonian rule that force was irrecoverably lost whenever a collision occurred between hard, inelastic bodies. The answer was, that nature had anticipated the objection and had avoided this kind of matter. Inelastic bodies were yielding bodies, and the force which had disappeared from the motion had done its work in changing the shape. But unless the body could recover its original figure by elasticity, there was no potential energy and force was annihilated. It is now believed, and to a large extent demonstrated, that the force, apparently lost, has been transformed into heat, electricity or some other kind of molecular motion, of which the change of shape is only the outward sign. The establishment on a firm foundation of theory and experiment of the so called conservation of energy, the child of the correlation of physical forces, is one of the first fruits of molecular mechanics.

It is no disparagement of this discovery, on which was concentrated the power of several minds, to call it an extension, though a vast one, of Newton's law of inertia, of Leibnitz's *vis viva*, and of Huyghens' and Bernouilli's conservation of living forces; these older axioms of mechanics having free range only in astronomy, where friction, resistance and collision do not interfere. The conservation of energy, in its extended signification, promises to be, like its forerunners, a valuable guide to discovery, especially in the dark places into which physical science has now penetrated. The caution which Lagrange has given in reference to similar mechanical principles, such as the conservation of the motion of the centre of gravity, the conservation of moments of rotation, the preservation of areas, and the principle of least action, is not without its applicability to the new generalization. Lagrange accepts them all as results of the known laws of mechanics and not as the essence of the laws of nature. The most that physical science can assert is that it possesses no evidence of the destructibility of matter or force.

It is not pretended that the existence of atoms has been or can be proved or disproved. Some chemists think that the atomic theory is the life of chemistry: others have abandoned it. Its importance is lost in that of the molecular theory. And what has this accomplished to justify its existence? If we define the mole-

cule of any substance as the smallest mass of that substance which retains all its chemical properties, we can start with the extensive generalization of Avogadro and Ampere, that an equal volume of every kind of matter in the state of vapor, and under the same pressure and temperature, contains an equal number of such molecules. The conception of matter as consisting of parts, which are perpetually flying over their microscopic orbits and producing by their fortuitous concourse all the observed qualities of bodies, is as old as Lucretius. He saw the magnified symbol of his hypothesis in the motes which chase one another in the sunbeam. One of the Bernouillis thought that the pressure of gases might be caused by the incessant impact of these little masses on the vessel which held them. The discovery that heat was a motion and not a substance, foreshadowed by Bacon, made probable by Rumford and Davy, and rigidly proved by Mayer and Joule when they obtained its exact mechanical equivalent, opened the way to the dynamical theory of gases. Joule calculated the velocity of this promiscuous artillery, rendered harmless by the minuteness of the missiles, and found that the boasted guns of modern warfare could not compete with it. Clausius consummated the kinetic theory of gases by his powerful mathematics, and derived from it the experimental laws of Mariotte, Gay-Lussac and Charles. By the assumption of data, more or less plausible, several mathematicians have succeeded in computing the sizes and the masses of the molecules and some of the elements of their motion. It should not be forgotten that mathematical analysis is only a rigid system of logic by which wrong premises conduct the more surely to an incorrect conclusion. To claim for all the conclusions which have been published in relation to the molecules the certainty which fairly belongs to some of them would prejudice the whole cause.

One of the most interesting investigations in molecular mechanics was published by Helmholtz in 1858. It is a mathematical discussion of what he calls ring-vortices in a perfect, frictionless fluid. Helmholtz has demonstrated that such vortices possess a perpetuity and an inviolability once thought to be realized only by the eternal atoms. The ring-vortices may hustle one another, and pass through endless transformations, but they cannot be broken or stopped. Thomson seized upon them as the impersonation of the indestructible but plastic molecule which he was looking for, to satisfy the present condition of physical science.

The element of the new physics is not an atom or a congeries of atoms but a whirling vapor. The molecules of the same substance have one invariable and unchangeable mass: they are all tuned to one standard pitch and, when incandescent, emit the same kind of light. The music of the spheres has left the heavens and condescended to the rhythmic molecules. There is here no birth or death or variation of species. If other masses than the precise ones which represent the elements have been eliminated, where, asks Maxwell, have they gone? The spectroscope does not show them in the stars or nebulæ. The hydrogen and sodium of remotest space are in unison with the hydrogen and sodium of earth.

In the phraseology of our mechanics we define matter and force as if they had an independent existence. But we have no conception of inert matter or of disembodied force. All we know of matter is its pressure and its motion. The old atom had only potential energy; the energy of its substitute, the molecule, is partly potential and partly kinetic. If it could be shown that all the phenomena displayed in the physical world were simply transmutations of the original energy existing in the molecules, physical science would be satisfied. Where physical science ends, natural philosophy, which is not wholly exploded from our vocabulary, begins. Natural philosophy can give no account of energy when disconnected with an ever present Intelligence and Will. In Herschel's beautiful dialogue on atoms, after one of the speakers had explained all the wonderful exhibitions of nature as the work of natural forces, Hermione replies: — "Wonderful, indeed! Anyhow, they must have not only good memories but astonishing presence of mind, to be always ready to act, and always to act, without mistake, according to the primary laws of their being, in every complication that occurs." And elsewhere, "Action, without will or effort, is to us, constituted as we are, unrealizable, unknowable, inconceivable." The monads of Leibnitz and the demons of Maxwell express in words the personality implied in every manifestation of force.

In this imperfect sketch of the increased resources and the present attitude of the physical sciences I have not aimed to speak as an advocate; much less to sit as a judge. The great problem of the day is, how to subject all physical phenomena to dynamical laws. With all the experimental devices, and all the mathematical appliances of this generation, the human mind has been baffled in

its attempts to construct a universal science of physics. But nothing will discourage it. When foiled in one direction, it will attack in another. Science is not destructive, but progressive. While its theories change, the facts remain. Its generalizations are widening and deepening from age to age. We may extend to all the theories of physical science the remark of Grote which Challis quotes in favor of his own: — "its fruitfulness is its correctibility." Instead of being disheartened by difficulties, the true man of science will congratulate himself in the words of Vauvenargues, that he lives in a world fertile in obstacles. Immortality would be no boon if there were not something left to discover as well as to love. Fortunate, thought Fontenelle, was Newton, beyond all other men, in having a whole fresh universe before him, waiting for an explanation. But science wants no Alexanders weeping because there are not other worlds to conquer. For every heroic Columbus, who launches forth, in however frail a bark, upon untried oceans, seeing before him rich continents where others behold only a wilderness of waters, there are precious discoveries in reserve. Surely the time has not yet come when the men in any section in this Association can fold their arms and say: — It is finished. Unless our physicists are contented to lag behind and gather up the crumbs which fall from the rich laboratories and studies of Europe, they must unite to delicate manipulation the power of mathematical analysis. Mathematics wins victories where experiment has been beaten. With good reason we applaud the many brilliant successes of instrumental research. Mathematical analysis, with its multitudinous adaptations, is the only key which will fit the most intricate wards in the treasury of science. With the help of her mathematical physicists, Great Britain has now taken a position in science which she has not held before since the days of Newton. In Germany, the physicists do not hold back from the most difficult problems of the day, because they are led along by experiment on one arm and by mathematics on the other. The zeal of the Italian scientists prevails, over even the terrors of Vesuvius, and makes them ready to become martyrs, like Pliny the elder, to nature and humanity. France, too, out of the very ashes of her humiliation, sends an inspiring word to us. Since her defeat, her scientific spirit has been aroused as it was after the days of the first revolution. Her Association for the Advancement of Science

is only a two year old infant; but it has sprung into existence, like Minerva from the head of Jupiter, fullgrown and equipped. Already it has displayed a vitality and a prosperity which this Association, in its opening manhood, has not yet acquired. The words of its first President are as true for the United States as for France: — that the strength and glory of a country are not in its arms but in its science.

REPORTS OF COMMITTEES.

Report of the Committee on the Preservation of Forests.

The Special Committee appointed at the last annual meeting of this Association, to memorialize Congress and State Legislature regarding the cultivation of timber and the preservation of forests, would respectfully report:

That on the 4th day of September following their appointment, a preliminary meeting was called, at the residence of Mr. George B. Emerson, in Boston, and an interchange of views was had upon that day, among a majority of the Committee, as to the most practicable means of carrying into effect the wishes of the Association implied in their appointment.

Being deeply impressed with the great importance of the interests depending, they at once felt that inquiries so comprehensive and varied required much more time and labor than individual members could well spare from their ordinary duties, and that before the merits of the subject could be pressed upon the attention of State Legislatures, it would be necessary to collect and prepare from a wide range of inquiry the facts upon which their action should be founded, so as to be able to suggest the methods and remedies that could be most effectually applied.

It was agreed that the subject should be brought directly before Congress, and a sub-committee, consisting of G. B. Emerson and F. B. Hough, was appointed on behalf of the committee to give personal attention to this duty. A few days afterwards a second meeting of a portion of the committee was held at the residence of Prof. Gray, in Cambridge, at which several others were present, and further discussion had, in harmony with the views expressed at the first meeting.

A memorial was soon afterwards prepared by the sub-committee and printed at the Salem Press. It was sent in form of circulars to each member of the committee, with a note inviting critical

examination and suggestions of amendment, and requesting return to Mr. Emerson in Boston. The type was kept standing several months, and several changes were made,—the document finally taking the following form:

After stating the appointment of the committee on the 22d of August, 1873, the resolution then adopted, and the organization of the committee, the memorial says:

"Under this appointment consultation has been had among members of this committee, who have requested the undersigned, on their behalf, to represent as follows:

That the preservation and growth of timber is a subject of great practical importance to the people of the United States, and is becoming every year of more and more consequence, from the increasing demand for its use; and that while this rapid exhaustion is taking place, there is no effectual provision against waste or for the renewal of supply.

We apprehend that the time is not distant when great public injury must result from this cause, and we deem it to be our duty to urge upon the Government the importance of taking timely action in providing against the evils that must otherwise follow.

Besides the economical value of timber for construction, fuel, and the arts, which is obvious without suggestion, and must increase with the growth of the nation, there are questions of climate that appear to have a close relation to the presence or absence of woodland shade. The drying up of rivulets, which feed our mill-streams and navigable rivers and supply our canals, the failure of the sources which supply our cities with pure water, and the growing tendency to floods and drought, resulting from the unequal distribution of the rainfalls since the cutting off of our forests, are subjects of common observation.

In European countries, especially in Italy, Germany, Austria, and France, where the injuries resulting from the cutting off of timber have long since been realized, the attention of governments has been turned to this subject by the necessities of the case, and conservative measures have in many instances been successfully applied, so that a supply of timber has been obtained by cultivation, and other benefits resulting from this measure have been realized.

Special schools of forestry have been established under the auspices of government, and the practical applications of science in the selection of soil and conditions favorable for particular species, and in the planting, care, and removal of timber, are taught and applied, with the view of realizing the greatest benefits at the least expense.

There is great danger that, if not provided against, the fearful changes may happen to our largest rivers which have taken place

on the Po and other large rivers of Italy, France, and Spain, caused by the destruction of the forests from which came their tributaries. These forests had retained the water from the snows and rains of winter and spring, and supplied it gradually during the summer. Since their destruction the rain falling in the rainy season is delivered almost at once, bringing with it earth and stones, deluging the banks of the larger streams, but leaving a very insufficient provision against the consequent drought of summer.

Thus, when the forests about the sources of our great rivers shall be cut away, the water from the melting snows and early rains will be liable to come down in vast floods, overflowing the banks and carrying ruin and destruction in their course, while the affluent streams in summer will diminish or disappear, to the great injury of the country through which they flow.

We deem it highly important that the true condition and wants of the country in this regard, and the injuries that may result from the destruction of the forests and the exhaustion of our supplies of timber, should be known in time to provide a remedy, before the evils are severely felt. There are facts of the greatest importance in relation to the past and present destruction of forests, the pressing want of timber trees in states without natural forests, and the changes that have taken place, or are taking place, in consequence of the destruction of the forests, that should be carefully collected and be widely and familiarly known.

A knowledge of these facts would be everywhere of great value. They should be gathered, arranged, and so widely published as to reach the intelligent inhabitants of all the states. There is not a State or Territory without a direct interest in the subject. We should know the experience of other countries, and be able to apply whatever may be found therein suited to our soil and climate, and consistent with the plan of our government and the theory of our laws.

Individual or associated effort, unless organized and directed by authority, could not be expected to conduct these inquiries, or make known the results with that fulness which the investigation would require. We therefore recommend them as worthy of the attention of Congress, as the immediate guardian of the Territories and the proper source of power in whatever concerns the interests of the whole country.[1]"

It will be noticed that the above memorial contains no suggestion as to a plan by which definite results might be attained. The necessity of some definite means by which these inquiries could be most effectually conducted was expressed in correspondence

[1] Among the injurious effects of the unequal distribution of rainfall, resulting from the destruction of forests, should be mentioned the wholesale sweeping away of the fertile surface soil, and the subsequent cutting of ravines into the sandy layers beneath, that has already assumed such alarming importance in the Gulf States, rapidly transforming both hill and valley into extensive tracts of arid sand, to a most alarming extent.

by several members of the committee, but without suggestion. It was therefore decided that this should be settled after the sub-committee should have had an opportunity of consulting persons at the seat of general government, whose familiarity with congressional proceedings, and whose interest in the subject would better enable them to advise upon this most important matter.

The two members to whom the duty had been assigned repaired to Washington late in January last, and, after consulting with the Secretary of the Smithsonian Institution and others, it was deemed advisable to invite several gentlemen in official places, and members of Congress known to be interested in the subject, to meet and discuss the practical details of the question. Such a meeting was accordingly held, at the rooms of Professor Henry, at the Smithsonian Institution, on the evening of February 2d, and the subject, after being carefully considered in its various aspects, was cordially commended to the attention of Congress. As the result of this, and further consultation, it was thought best to ask for the passage of a law analogous to those creating the commissions of mining and of fisheries, which have unquestionably tended greatly to advance the interests they were intended to promote. The following addition was accordingly made to the memorial:

"We would therefore respectfully request the passage of a law creating a Commission of Forestry, to be appointed by the President and Senate, and that it should be required to ascertain, from the most effectual and reliable means within its power, and to report to Congress upon the following subjects:

First. Upon the amount and distribution of woodlands in the United States, the rate of consumption and waste, and the measures that should be adopted to provide against the future wants of the country in the preservation and planting of timber. With this there should be an inquiry concerning the importation and exportation of lumber and other forest products:

Second. The influence of forests upon the climate, and, especially, as to what extent their presence or absence tends to affect the temperature, rainfall, and other atmospheric conditions upon which agricultural success depends:

Third. A full statement of the methods practised in Europe in relation to the planting and management of forests, and an account of the special schools of forestry that have been established in foreign countries."

The Senators and Representatives who were consulted upon the subject unanimously agreed in recommending that a reference should be asked in each House, to the Committee on Public Lands.

These committees had already, under consideration, a bill which soon afterwards became a law, amending the act of the last previous Congress for the encouragement of tree-planting on the public lands, and the members that composed them were known to be well informed concerning the interests depending upon the questions involved in the memorial.

It was furthermore decided to lay the subject fully before the Commissioner of the General Land Office, the Secretary of the Interior and the President. Each of these officials cordially approved of the measures recommended, as the most effectual for collecting and utilizing the information needed in promoting the objects for which the committee had been appointed, and which they regarded as of the highest importance to the future welfare of the country. It was readily agreed that the memorial should be presented to Congress, endorsed by these high officials, with a special message from the President addressed to each House, accompanied by the draft of a joint resolution designed to give effect to the recommendation in the memorial.[2]

This message was referred to the desired committee in each House. At this stage of the proceeding, the senior member of the sub-committee was recalled by private affairs, and a delay of some weeks occurred, before the subject could be reached in its order in the committees; the interval being employed by his colleague, in condensing from authorities in the libraries of Congress, and the departments, such facts as appeared to justify and confirm the statements of the memorial. It was meanwhile agreed between the two committees, that the House committee to which it had been first referred, should make a detailed examination and report, and that the Senate committee should reserve its action until this report was printed.

The subject being reached in its turn by the House committee, an opportunity for hearing was given, and the question was referred to a sub-committee, consisting of the Hon. Mark N. Dunnell of Minnesota, as chairman, and Messrs. Wm. A. Phillips of Kansas, and W. S. Herndon of Texas. These gentlemen faithfully examined the subject in its details, and prepared the following report, which being adopted as that of the whole committee was duly presented to the House, on the 17th of March last:

[2] Ex. Doc., No. 28, 43d Congress, 1st Session, Senate.

"The Committee on the Public Lands, to whom was referred the special message of the President of the United States communicating information in relation to the cultivation of timber and the preservation of forests, together with a communication from the Commissioner of the General Land Office upon the same subject, would respectfully report:

That they have given their attention to the subject, and learn that the memorial above referred to was prepared by a committee appointed by the American Association for the Advancement of Science, as the result of a discussion induced by a communication read before them at their annual session in Portland, Maine, on the 21st day of August, 1873, "On the duty of governments in the preservation of forests."

A subject indorsed by an association embracing within its membership the highest scientific talent of the country must commend itself to our notice as worthy of attention. More especially is this notice due, when their action takes the form of a recommendation to Congress upon a subject alleged to involve the duty of the Government upon questions that vitally affect the interests of the whole country, and especially those of agriculture, manufactures and commerce. When it is further affirmed that, without timely provision by law, these great agencies of civilization and elements of wealth will, in the near future, be materially impaired, we cannot hesitate to give these recommendations a most careful examination, to the end that, if well founded, the measures best calculated for averting these injuries may be devised, and the remedies most effectually applied.

After as full an investigation of this question as present opportunities allow, we are convinced that the statements of the memorial are essentially true, and that it is the duty of the Government to take immediate measures for ascertaining the condition and prospects of our timber supply, to the end that the future wants of the country with regard to these great interests, both in their scientific and practical relations, should be thoroughly investigated and made widely known.

In European countries, large forests are owned and managed by governments, and elaborate systems of culture have been devised. With us, the greater part of the lands in the older States have already passed into the hands of private owners, and much that remains in the newer States and in the Territories is without timber. The operations of planting and management must, therefore, be left to private enterprise. We do not now recommend the undertaking of this industry by the Government, nor can we foresee that such a measure could ever in future be attempted beyond the reservation of such valuable timber-tracts as remain in our public domains, and that may seem to require preservation against injury or depredation.

But, in the great questions of forest-economy which we see arising, and concerning which our people have everything to learn, we realize the need of every aid to practical success that the experience of older nations in Europe has wrought out, and all the benefits that science can confer, in rendering success in forest-culture and timber-economies certain and complete.

There is no way in which this can so effectually be done, as by the employment of a man thoroughly competent for the duty, who might be able to bring to his subject the fruits of ripe experience in statistical inquiries and scientific labor, and collect, classify, condense, and make available whatever knowledge there may be found appertaining to the general object. And being deeply impressed with the great importance to the whole country of such a measure of economy and utility, and of the necessity for early action, your committee recommend the accompanying bill, H. R. 2497. A bill for the appointment of a commission to inquire into the destruction of forests, and into the measures necessary for the preservation of timber.

We also adopt and make a part of the report, the letter of Dr. Franklin B. Hough, chairman of the committee of the American Association for the Advancement of Science upon the subject of forestry and his very able and scientific analysis of the subject of forestry, made an appendix hereto."[3]

The first draft of the bill was somewhat modified by the committee, with the design of rendering it more definite in form, and some progress having been made, it was placed in the hands of a member of the committee on public lands, who had taken special interest in its success, but a favorable opportunity for bringing it to a final vote did not occur during the session.

In the Senate committee a hearing was granted and favorable assurances were given by most of the members present, although no formal action was taken, it being thought proper to await the action of the House, which if favorable would bring it directly before them.

Your committee are impressed, from a study of this subject, with the great and growing importance of the timber question, as one that should early engage the attention of our people and invite all the aid that science can impart. As well remarked by the Congressional committee who have had this interest under consideration, we should not expect that our general or state governments will ever directly undertake the planting and preservation

[3] House Report, No. 259, 1st session 43d Congress. Of this Report, which with accompanying papers makes a document of 120 pages, an extra edition of 5000 copies was printed by order of the House.

of forests. They may by judicious reservation protect the timber on lands still owned by them, and they may by carefully devised laws, invite and protect the owners of the soil in undertaking this industry at their own expense, and by punishing trespass and enforcing precautions against running fires, may thus directly promote the growth of timbers. But mainly is this result to be sought through an intelligent, systematic and well considered series of statistical and scientific inquiries, tending to ascertain and make widely known whatever is most valuable for practical application, and with means to call into service the highest talent of the country in special points of investigation, as occasion may, from time to time, require. The adaptation of species to climate, soil, and circumstances should be carefully studied and verified by comparison of an ample collection of facts, to the end that effort be not wasted in unprofitable experiment, but turned to the planting that shall yield the greatest profit, at least expense of time and labor. The advantages offered in the introduction of foreign species, so encouragingly shown in the recent cultivation of the *Eucalyptus* in southern Europe, Algeria and California, should induce researches on a liberal scale, with the view of gaining any benefit within our reach, from the acclimation of timber having valuable uses and qualities. An extension of knowledge as to the methods of management, by which the best results are attained in European forestry, would place at our service the benefits of years of experiment and careful observation by those who have been trained with the utmost care in this science. The success with which shifting sand-dunes have been fixed, and barren heaths reclaimed by tree planting in other countries affords suggestions of great practical interest in our own; and the discovery of causes tending to the destruction of forests, whether from insect ravages or disease, must necessarily precede a knowledge of the means by which these injuries may be prevented.

Much has been said, both in this country and in Europe, as to the relation between forests and climate, the greater number affirming, but some denying, that the cutting off of forests increases the tendency to floods and drought, and that the planting of trees tends to increase the frequency of the fall of rain. It is needless to remark that nothing short of a long and carefully conducted series of observations within, near and apart from forests, will

ever settle the true facts of the case to the satisfaction of all parties.

This inquiry has been undertaken in the true spirit of inductive philosophy by Prof. Ebermayer of the Central-Forstlehranstalt at Aschaffenburg, Bavaria, and the first results are already known to the scientific world. Stations have been established in various countries of Europe, and in a communication to the chairman of your committee he expresses a hope that comparative observations may be made in America. The subject has been mentioned to the chief of our weather-signal service, and assurances received that the desired observations will be established at stations favorable for the purpose, whenever means are provided for the moderately increased expenses which they would occasion.

Your committee do not deem it necessary to insist upon the fact, that our supplies of timber are being rapidly exhausted, and that the time is not many years distant, when we must begin to feel its want, as a material for building, and for use in the arts. From estimates made by experienced lumbermen, the total supply in the forests available for lumber in the United States and Canada is between 250 and 300 billions of feet, while the annual consumption is not less than 20 billions, and is rapidly increasing. With this consumption and waste, we should include the destruction caused by supplying bark for the tanneries, the demand for railroad construction and use, and the enormous quantities used annually as fuel, and destroyed by forest fires.

It is not wise to wait till the supplies are wholly exhausted, and we should at once begin to study the economies of forestry, and the means by which these supplies may be preserved and renewed. Believing that this result may be best gained, in the manner above set forth, we respectfully submit this statement of proceedings, and these views as to duties, and ask a continuance of the powers of the committee, in the hope of being able, at a future time, to report more effective results from our labors.

FRANKLIN B. HOUGH,
GEO. B. EMERSON,
ASA GRAY,
EUG. W. HILGARD,
WM. H. BREWER,
Committee.

REPORT OF THE COMMITTEE ON ACT OF INCORPORATION.

THE Committee appointed by the Association at the meeting in Portland, in August last, to obtain an Act of Incorporation for the Association, have the honor to report :—

The members of the committee, having been duly notified, held a meeting on Friday, October 17, 1873, at the rooms of the Academy of Arts and Sciences, Boston. In the notice for the meeting, members who were unable to attend, were invited to communicate in writing to the meeting any views or suggestions which they might have or wish to make in reference to the subject referred to them.

Three members of the committee were present at the meeting, and letters were received from others stating their inability to be present, and it was suggested by one of them that "all the advantages of a full meeting might be secured by means of correspondence." It was not deemed necessary to organize the committee in any formal manner and after a brief discussion upon the subject, on motion of Prof. Gray, Mr. Wheildon was requested to prepare a suitable application for a charter and to present the same at the then approaching session of the legislature at Boston. Accordingly a suitable petition was prepared, which bore the names of all the committee. This petition was presented in January and was referred to the Committee on Education, composed of members of the two branches of the legislature. By arrangement with the committee a time for a hearing was appointed to be held at the State House, on Tuesday, 24th of February, 1874, at 11 o'clock.

In the meantime the draft of a bill had been prepared, of which printed copies were sent to all the members of the committee; and these, excepting one copy, were returned, with the approval of the members respectively. The members of the committee whom it was supposed might be able to attend the hearing were duly notified of the time and place and desired to be present. Messrs. Putnam and Wheildon gave to the committee of the legislature a brief sketch of the history of the Association, its character and objects, the number of its members, and its purposes in desiring a special act of incorporation. The committee made a particular request that the names of all the living past and present Presidents of the Association should be inserted as corporators in the proposed charter—regretting the necessity of

omitting in this honored list the names of Redfield, Bache, Agassiz, Torrey, Foster and others. The suggestion was kindly assented to; and such inquiries as the committee of the legislature desired to make were satisfactorily answered by the members of the committee of the Association who were present. The draft of the act which had been prepared was then submitted to the committee, and, with the omission of some minor provisions which, it was found, are fully covered by the general statutes of the Commonwealth, was adopted.

On the 26th of February the bill was reported in printed form in the senate. It was subsequently passed through the various stages in both branches of the legislature and received the signature of the Governor of the Commonwealth on the 19th day of March following.

An official copy of the act attested by the Deputy Secretary of the Commonwealth, together with copies of the other papers mentioned in this report, are herewith presented. The act makes the Association a corporation for the purpose of holding, receiving and conveying real estate and personal property, "with all the powers and privileges, and subject to the restrictions, duties and liabilities set forth in the general laws which now or hereafter may be in force applicable to such corporations." The 2d section of the act authorizes the corporation to "hold by purchase, grant, gift or otherwise, real estate not exceeding one hundred thousand dollars in value, and personal estate of the value of two hundred and fifty thousand dollars." Section 3d authorizes any two of the corporators to call the first meeting of said corporation "in the month of August next ensuing, by notice thereof by mail to each member of said Association," and this provision has been complied with by an official notice signed by Joseph Lovering and John L. LeConte, two of the gentlemen named in the act.

Respectfully submitted on behalf of the Committee,

Wm. W. Wheildon.

Concord, Mass., August 7, 1874.

REPORT OF THE COMMITTEE ON MRS. THOMPSON'S DONATION.

In accordance with the duties assigned to them, the Committee have accepted the Memoir by Mr. Scudder, on Fossil Butterflies, as the first paper to be published by the Thompson Fund, and while regretting that the unavoidable delay in engraving the plates prevents their having the gratification of presenting the work at the present Meeting, they believe that the Association and its liberal patroness will accept the Memoir as one in every way worthy of the honor thus bestowed.

ASA GRAY,
JAMES HALL,
THOMAS HILL,
P. H. VAN DER WEYDE,
J. L. LE CONTE,
T. STERRY HUNT,
F. W. PUTNAM,
Committee.

PAPERS READ

AT THE

HARTFORD MEETING.

A. MATHEMATICS, PHYSICS AND CHEMISTRY.

ON THE INNER SATELLITES OF URANUS. By EDWARD S. HOLDEN, of Washington, D. C.

IT is well known that Sir William Herschel suspected that the planet which he had discovered was accompanied by six satellites. These he numbered in their order, proceeding outwards from the planet:

I.	Period,	5	days,	21	hours.
II.	"	8	"	18	"
III.	"	10	"	23	"
IV.	"	13	"	11	"
V.	"	38	"	01	"
VI.	"	107	"	16	"

Of the existence of satellites II and IV there is no manner of doubt since they were steadily observed by the elder Herschel from 1787, Jan. 11, to 1810, May 25, and by his son in the years 1828 to 1832, as well as by Lamont, Struve, Lassell and Newcomb since that time, and all of these observations have been consistent.

Satellites I, III, V and VI have no such evidence in favor of their existence as the brighter ones II and IV.

From the "Philosophical Transactions," 1815, where Herschel has collected all his observations of the satellites of Uranus, it appears that he supposes satellite I to have been observed on four different occasions, viz.:

Jan. 18, 1790,
Mar. 27, 1794,
Feb. 15, 1798,
Apr. 17, 1801.

Satellite III was seen only twice, on March 26 and 27, 1794, while V and VI were suspected at various times.

Lassell in 1847 discovered two satellites interior to Herschel's II, and we owe to him and to his assistant Marth a good series of observations of all four satellites. The periods as determined by Lassell are

	d.	h.	m.	s.
Ariel =	2	12	29	20·7
Umbriel =	4	03	28	07·5
Titania =	8	16	56	25·6
Oberon =	13	11	06	55·4

It is evident that Herschel's II and IV are Lassell's Titania and Oberon.

Lassell's station at Malta was much better in regard to clearness of sky than Herschel's in England, his instrumental means was far superior, and the altitude of Uranus was greater at Malta in 1864 than in England in 1798, so that we must assume that if Lassell could not see Herschel's I, III, V and VI they did not exist.

In his report of his observations (Memoirs R. A. S., vol. 36) Lassell says that he repeatedly scrutinized the vicinity of the planet for the purpose of detecting faint satellites exterior to Oberon, and that he never suspected the existence of any such. Therefore, in the examination of Herschel's observations I shall reject all those referring to satellites V and VI, and for the same reason I shall reject all those referring to satellite III; in this latter case we have the added testimony of five months' observations with the Alvan Clark refractor of the U. S. Naval Observatory at Washington.

There remain then of Herschel's observations only those of suspected interior satellites which it will be profitable to examine.

Before selecting any of these observations for discussion it is necessary to premise a few words in regard to Herschel's method of observation. On a very few occasions he was able to faintly illuminate the wires of his micrometer for a determination of the position of Oberon and Titania, but *all* of his estimations of the position of any small objects interior to these had to be made in a perfectly dark field. Hence these estimations are liable to a large error of from five to fifteen degrees in position angle. Owing to the glare of the planet in the field of the telescope,

Herschel found that he could seldom see Oberon nearer to the planet than 23″.6, while Titania was usually invisible at distances less than 18″.1.

Of course under ordinary circumstances Ariel and Umbriel could not be visible at all, but there were occasions when the fine polish of his mirror or the good state of the atmosphere permitted him to view objects even as close as 10″. It was evidently impossible for him to see an interior satellite on two consecutive nights and of this he was fully aware.

It was his habit to make his observations of the "first" and "second" satellites as he calls them in his notes (*i.e.*, of Titania and Oberon) and to map down all small stars near to the planet. On the next subsequent observing night he examined the spot where the planet had been and was thus able to identify all small stars as such. In his printed observations the sketches of star-configurations are not given but his remarks in full are quoted, followed by an "identification" as he calls it, of all suspected satellites. The patience and skill with which these identifications are carried out year after year are truly admirable, and they give a real value to that which without them would be simply a ponderous mass of useless material.

I have selected from his observations as printed, *all* cases where he has seen an object interior to Titania sufficiently well to allow him to give an estimate of its position, excluding of course all cases where he has subsequently proved that such an object was certainly a star.

The cases for examination are

1.	1787.	Feb. 10 .	Position of interior satellite	135°
2.	1790.	Jan. 18	"following"	
3.		Jan. 20		315°
4.	1793.	Feb. 5		250° 57′
5.		Mar. 9		205°
6.	1794.	Feb. 28		66°
7.		Mar. 27		——
8.	1798.	Feb. 15		5° 11′
9.	1801.	Apr. 17	189° . .	distance 18″

The elements which I have used are provisional ones derived by Prof. Newcomb from Lassell's Malta observations. They are amply adequate to the present inquiry.

From these elements I have computed the angle of position and distance of Umbriel and Ariel in each of the cases above set down and compared these with Herschel's observations, as follows:—

1. "1787. Feb. 10, 8^h 57^m. The first satellite is about 53° n. p. 8^h 33^m * * * * * * * a supposed third is about 45° s. f. In a little more than four hours I saw the satellites go on with the planet and also in their orbits * * * no subsequent observation of the third was made."

On this date Umbriel was in P=82° distant 16″ and Ariel was n. p. Hence Herschel's "supposed third" was neither of the inner satellites.

2. "1790. Jan. 18, 9^h 32^m. There is a supposed third satellite about two diameters of the planet following, extremely faint and only seen by glimpses: 1^h 6^m after I could not perceive it: a fourth is about 70° n. p."

"Two diameters" of the planet was about 8″, and as Herschel usually counted his distance from the *limb* of the planet in his estimations, the distance from the centre would be about 10″. Umbriel was in P = 124° 23′, and distant 13″.9. Ariel was in P = 317° 10′, distant 10″.96. "1^h 6^m after" Umbriel was in P = 118° 48′, and distant 13″.54; *i.e.*, nearer to Uranus by 0″.4. So far as the evidence goes we may reasonably infer that Herschel had a glimpse of Umbriel. In the "Phil. Trans.," 1798, p. 271, Herschel in referring to this observation speaks of it as very certain and supposes that the satellite might have been "11 or 12 degrees" from the parallel. The above identification it is true brings it 34° from the parallel, but we must remember that in the first place the angle of position was merely an estimate, and secondly that "following" is here shown by Herschel himself to have been but a rough term, not indicating an angle of position of exactly 90°.

The "fourth satellite" of this night was proved to have been a star.

3. "1790. Jan. 20, 12^h 5^m, * * * a third satellite is 45° n. p. in a line with the planet and second satellite."

Umbriel had a position angle of 301° and was distant 13″.72; Ariel was n. f.; Oberon (the "second satellite") was in P = 324° 08′, and we must again conclude that Umbriel was seen.

4. "1793. Feb. 5, 9^h 18^m, * * * a very small star is 19° 3′ s. p. * * * There is no subsequent observation of the small star."

Umbriel was in P = 187° 19′ and Ariel was n. p. Herschel's "small star" had a position angle of 250° 57′, and hence it was neither Ariel nor Umbriel.

5. "1793. March 9, 10^h 35^m, * * * a third [satellite] is about 65° s. p."

Umbriel was n. p. and Ariel was n. f., and hence this observation refers to neither of them.

6. "1794. Feb. 28, 9^h 43^m. * * * There is a small star * * * about 24° n. f." Both Ariel and Umbriel were n. p., and hence the small object was a star.

7. "1794, March 27. A supposed third of this evening is preceding the first satellite, but nearer the planet. * * * The first satellite was 79° n. f."

Titania (the "first satellite") was in P = 10° 20′, distant 34″.43, while Ariel was in P = 20° 48′ and distant 13″.48, Umbriel being at this time s. f. Hence we must conclude that Herschel saw Ariel.

8. "1798. Feb. 15, 12^h 13^m, * * * position of the supposed 5th satellite" (which was really Oberon) "84° 49′ n. f. * * * at "about half the distance of the second satellite," and between it and the planet Herschel saw "what must have been an interior satellite at its greatest northern elongation." Oberon was in P = 14° 46′, distant 32″.00; Ariel was in P = 213° 7′, distant 5″.06, and therefore invisible; Umbriel was in P = 194° 26′, distant 18″.99, and therefore in its most favorable position. Herschel says the interior satellite was between Oberon and the planet, and if this is so he did not see Umbriel. His account of this night's work (*op. cit.*, pp. 332–3 and 359) is confused and leads to the suspicion (no more) that an examination of the originals might prove his position of the interior satellite 180° wrong—in which case Umbriel would have been seen; as it is, we must suppose the contrary.

9. "1801. April 17, 10^h 30^m. There is a third satellite at a great angle south preceding; in the configuration it is marked exactly in opposition to the second and at half the distance of the first. * * * The third by the configuration was 81° s.p." On the next night Herschel examined the place where the planet was on

April 17 and found no star in the former place of the third satellite. Herschel's satellite was in P = 189°, distant 18". Umbriel was in P = 191° 27', distant 21".18. Hence Herschel saw Umbriel.

The above are all the cases which a careful examination of the printed observations suggests for discussion, and it is to be remarked that of the four cases where Herschel supposed he saw the interior satellites, three have been verified fully, and a reasonable suspicion exists that the fourth may have been likewise a veritable observation of Umbriel. A reference to the original would probably settle most of the doubts which have arisen.

We may conclude then that the elder Herschel was in truth the first observer of Ariel and Umbriel as well as of Titania and Oberon, but that he was unfortunately prevented from identifying the inner satellites because his telescope could not show them on two successive nights.

It is to be noted that Sir John Herschel never caught a glimpse of them during his examination of Uranus with the same telescope in 1828–1832; the extreme difficulty of these objects makes us wonder at the marvelous skill and patience manifested by the elder Herschel in this delicate research.

It would be an interesting and useful research to endeavor to explain Herschel's observations of the III, V and VI satellites and to show that these were observations of small stars. This research I hope to execute upon the return of Prof. Watson and Dr. C. H. F. Peters from their respective journeys for the purpose of observing the Transit of Venus. Each of these astronomers has the most extended and minute maps of all the small stars in the region where Uranus was in 1787–1801; and a careful examination of tracings of these maps and of Herschel's observations could not fail to throw some light upon the supposed discovery of satellites III, V and VI.

The next observations of these objects were by Lassell and O. Struve in 1847.

Lassell's observations are given in "Mon. Not. Royal Astronomical Society" vol. viii, p. 44. I have compared these with the theory as beyond:—

Date.	Lassell's Pos'n of Satellite.	Position of Umbriel.	Position of Ariel.
1. 1845, Oct. 5,	324°.1	bet. 0° and 90°	bet. 0° and 180°
2. 1847, Sep. 14,	350°	bet. 0° and 90°	P = 345°.4 Δ = 13″.96
3. 1847, Sep. 27,	326°		P = 320°.1 Δ = 10″.45
4. 1847, Sep. 29,	336°	P = 173°.3	P = 1°.3 Δ = 13″.27
5. 1847, Oct. 1,	348° Δ = 18″.44	P = 354°.8 Δ = 19″.10	
6. 1847, Nov. 6,	80° Δ = 10″	P = 141° 36′ Δ = 15″.4	
" " "	349° Δ = 11″		P = 350°.1 Δ = 13″.89

Hence it is plain that Lassell saw Ariel on Sep. 14, Sep. 27, and Nov. 6, and possibly on Sep. 29. Umbriel was seen on Oct. 1.

Struve's observations are given in Mon. Not. R. A. S., vol. viii, p. 46.

Date.	P. of Umbriel (Struve).		P. of Umbriel (calculated).
1. 1874, Nov. 1	P = 194° Δ = 17″.8		P = 186° 47′ Δ = 16″.59
2. " Nov. 28	203.6	17.0	bet. 0° and 90°
3. " Dec. 9	218.6	13.7	P = 152° 54′ Δ = 17″.4, while Ariel was bet. 270° and 360°
4. " Dec. 10	180.1	17.0	P = 169° 38′ Δ = 19″.35
5. 1848, Jan. 25	202.0	18.0	bet. 0° and 90°

Hence Struve may have seen Umbriel Nov. 1, and Dec. 10, 1847.

Lassell's long series of Observations at Malta and Valleta has been previously mentioned.

The next series of observations of Ariel and Umbriel was made at Bothkamp in 1871, with a telescope of twelve inches aperture, and the results were compared with Marth's ephemeris for that year. The positions of Ariel differ from their predicted positions by a large angle, nearly 180°, while the positions of Umbriel agree well.

Ariel, however, is much the brighter of the two inner satellites and as it evidently was not seen at all, it becomes probable that a small star was mistaken for Umbriel on the five nights of observation.

This supposition is strengthened when we consider that the Bothkamp observers found Titania and Oberon difficult objects, which

they certainly are not to any telescope which will show Ariel or Umbriel. Lassell estimates that Oberon and Titania are twice as bright intrinsically as either of the inner satellites, and this estimate is probably not too high.

We may then fairly claim that Sir William Herschel saw all four of the satellites of Uranus, that Lassell discovered independently and first of any of his contemporaries the two faint satellites, while Struve probably saw Umbriel on one or two occasions; and that the inner satellites have not all been seen with any telescopes save the twenty and forty foot reflectors of Herschel, the telescopes of Mr. Lassell (two and four foot reflectors) and by the Clark refractor at Washington.

The great reflecting telescope of Dr. Henry Draper, the Clark refractor of Chicago, and Mr. Newall's Cooke refractor, have never been used upon these objects so far as I know, although all three are undoubtedly adequate to the research.

Some Improvements on Descartes' Barometer. By Benj. S. Hedrick, of Washington, D. C.

The Descartes' barometer has the tube enlarged in the upper portion, the enlarged portion extending a little below that portion which would correspond to the lowest point of the mercury barometer. Above the enlarged portion is a continuation of a small tube. Two fluids are used, the lower being mercury and the upper a lighter liquid, such as water or oil. In theory this barometer has the advantage of giving a much larger rise and fall corresponding with changes in the weight of the air; but in practice it has not proved useful, for the reason that water will contain air which is given off and spoils the vacuum. Spirits and other light liquids give off vapor by rise in temperature and the ordinary oils oxidize.

I propose to substitute for the light liquid heavy coal oil or petroleum. This can be obtained of a uniform boiling point of about 300° Fahr. free from paraffine. It will not affect the surface of the mercury.

By this change the Descartes, the Huyghens, the Hooke and other two-liquid barometers will be made practicable. As the range of rise and fall is so much increased, the indications may be easily read by persons with little skill, and when the photographic method of automatic recording is employed good results may be obtained.

MORTALITY IN EACH YEAR AMONG THE OFFICERS OF THE ARMY (INCLUDING THOSE KILLED IN BATTLE) FOR FIFTY YEARS, FROM 1824 to 1873, AS DERIVED FROM THE ARMY REGISTERS. By BENJ. ALVORD, Paymaster General, U. S. Army.

DATE OF ARMY REGISTER.	NUMBER OF OFFICERS AT COMMENCEMENT OF YEAR.	NUMBER OF DEATHS IN EACH YEAR, INCLUDING WAR RISKS.	PER CENT ANNUALLY.	ONE IN THE NUMBER GIVEN DIED PER YEAR.
1824	548	8	·01459	1 in 68
1825	561	18	·03209	1 " 31
1826	570	8	·01403	1 " 71
1827	583	11	·01887	1 " 53
1828	563	16	·02842	1 " 35
1829	590	8	·01356	1 " 74
1830	616	7	·01136	1 " 88
1831	634	8	·01262	1 " 79
1832	583	24	·04117	1 " 24
1833	675	5	·00741	1 " 135
1834	671	23	·03428	1 " 29
1835	675	22	·03259	1 " 31
1836	676	33	·04882	1 " 20
1837	627	22	·03519	1 " 28
1838	723	18	·02489	1 " 40
1839	702	24	·03419	1 " 29
1840	722	14	·01939	1 " 51
1841	733	20	·02728	1 " 36
1842	786	16	·02036	1 " 49

DATE OF ARMY REGISTER.	NUMBER OF OFFICERS AT COMMENCEMENT OF YEAR.	NUMBER OF DEATHS IN EACH YEAR, INCLUDING WAR RISKS.	PER CENT ANNUALLY.	ONE IN THE NUMBER GIVEN DIED PER YEAR.
1843	814	4	·00491	1 in 203
1844	835	9	·01078	1 " 93
1845	846	19	·02246	1 " 44
1846	860	37	·04302	1 " 23
1847	939	80	·08519	1 " 12
1848	972	24	·02469	1 " 40
1849	962	36	·03742	1 " 27
1850	964	20	·02075	1 " 48
1851	1,003	23	·02293	1 " 44
1852	993	16	·01611	1 " 62
1853	979	43	·04392	1 " 23
1854	1,002	25	·02495	1 " 40
1855	996	22	·02208	1 " 45
1856	1,051	15	·01423	1 " 70
1857	1,066	19	·01773	1 " 56
1858	1,097	24	·02188	1 " 46
1859	1,102	15	·01361	1 " 73
1860	1,100	16	·01455	1 " 69
1861	1,813	24	·01324	1 " 75
1862	1,634	103	·06304	1 " 16
1863	1,943	89	·04581	1 " 22
1864	1,901	92	·04839	1 " 21
1865	1,761	57	·03237	1 " 31
1866	2,205	67	·03038	1 " 33
1867	3,000	67	·02233	1 " 45
1868	3,027	21	·00694	1 " 144
1869	2,903	11	·00378	1 " 264
1870	2,876	53	·01843	1 " 54
1871	2,435	48	·01971	1 " 51
1872	2,404	31	·01289	1 " 77
1873	2,411	46	·01908	1 " 52
AVERAGE.	1,203	29	·02410	1 " 41

From this it appears that the annual rate of mortality among

officers of the U. S. Army for fifty years, from 1824 to 1873, has been 24·1 per 1,000.

The Florida war extended from 1836 to 1841; the Mexican war from 1846 to '48; the civil war from 1861 to '65. These years are plainly discernible in the table by the increased rate of mortality. But it is interesting to see that this mortality is not excessive through a long period of time. The rate of mortality in the United States for 1870, according to Mr. E. B. Elliott (see page 10, "Vital Statistics of the Census"), was 21·6 per 1,000.

The annual rate of mortality in England for thirty-three years, from 1838 to 1870, was 22·4 per 1,000; of males in England in 1870, 24·3. The death rate of the United Kingdom in 1870 was 23·9, and of officers of the British Army, stationed in Great Britain in 1870, was 16·6 per 1,000. (See Report of Register General of England, pages 13, 14, and 50.)

The death rate in France from 1859–68, ten years, was 23·3 per 1,000. In 1870, it was 28·8, owing doubtless to the Franco-German war.

In Austria the death rate in 1870 was 29·2 per 1,000, against 29·3 and 28·9 in the two preceding years. The English death rate in 1870 was 23 per 1,000, or 6·2 less than in Austria.

In Spain the death rate in 1870 was 30·1 per 1,000.

From the above table it appears that the annual death rate of officers of the army in the period of twenty-five years, from 1824 to 1848, was 27 per 1,000; the rate for twenty-five years, from 1849 to 1873, was 23 per 1,000, showing a decided decrease notwithstanding the civil war.

From the statistics of "The English Life Table," by William Farr, page 18, I calculate that the average annual death rate of males over twenty-one years of age in England and Wales, from 1841 to 1851, was 21 per 1000. The exact fraction reached was ·02104.

ON THE USE OF A GLAZED WROUGHT IRON COMBUSTION TUBE FOR NITROGEN DETERMINATIONS. By A. P. S. STUART, of Clinton, Mass.

IT is well known that an accurate determination of N in a nitrogeneous compound is one of the difficult tasks of the analytical chemist. Leaving out of view the method of Varrentrap and Will as very questionable in point of accuracy, as well as several others, depending on the conversion of nitrogen into ammonia, our effort has been to obtain an accurate and expeditious method of determining t e absolute quantity of nitrogen by means of the Sprengel pump. The two important difficulties which we have sought to obviate, and which, so far known, are incident to all methods of determining nitrogen by volume, are, first, the liability of glass combustion tubes to fracture, and secondly, the difficulty of making air-tight connections. The first of these difficulties we sought to obviate by substituting a wrought iron, for a glass combustion tube, and the second, by connecting the combustion tube with the T of the pump by means of a screw. Such an arrangement, of course, presupposes a peculiar construction of the T of the pump. This was so formed as to terminate with an external screw and shoulder. For a combustion tube a piece of ordinary iron gas tubing of the proper length and diameter was sealed at one end, and at the other an internal screw was cut of the exact size to fit the T of the pump. Thus constructed, the tube, by the use of a little gas-fitter's cement, could be screwed on to the T of the pump in a few moments so closely, that when it was freed from air, and its temperature unchanged, the mercury in the tube of the pump would remain at the barometric height for an indefinite length of time, indicating thus a Torricellian vacuum.

To perform perfectly, however, the functions of a combustion tube, the mercury shou d not sink in the tube of the pump, when the temperature of the combustion tube was raised; or, if it did sink, it should rise again to its original height on the cooling of the tube to its original temperature. Such, however, was not the fact. On the application of heat by means of a Hofman's gas furnace, the mercury was observed to sink gradually, even before the combustion tube was at a dull red heat, and on the cooling of the tube, not to return to its original height in the pump. The inference was, that gaseous matter of some kind had entered the

combustion tube, in consequence of its having been heated, and that this gaseous matter had either been occluded in the iron, or had actually passed through the pores of the iron on being heated. On pumping out this gaseous matter and collecting it in a test tube over mercury, it was found to explode on the application of a flame. It was, therefore, hydrogen, with perhaps, a little carbon monoxide, or carburetted hydrogen. It occurred to us then, that iron, like platinum, palladium, gold and copper, might have been found by Graham to be pervious to hydrogen at a high temperature, and on reviewing his researches on the subject, we found, that such was the fact, and that it had unfortunately escaped our memory.

To obviate this difficulty of the porosity of iron at a high temperature for hydrogen, which, for a time, seemed insuperable, tubes of steel and brass were successively employed. The porosity of these substances for gases, so far as we know, had never been experimented on by Graham, and the thought was natural, that, possibly the pores of iron might be so filled with carbon, and those of copper with zinc, as effectually to prevent the passage of gases through these substances. Experiments, however, in these cases were followed by scarcely better results. As soon as the tubes were heated to a temperature a little below a dull red heat, the mercury began to fall; and when the gaseous matter was pumped out, and collected over mercury, it proved, as did that from iron, to be explosive.

Although our object was not to determine the nature of gases that pass through metals or alloys at a high temperature, still curiosity prompted us to collect the gas from the steel tube in a eudiometer, and to test it after explosion, for carbon in the usual way with caustic potash. The ascent of the mercury in the eudiometer showed conclusively the presence of CO_2. Some gaseous compound of carbon, therefore, probably CO, or a carbide of hydrogen must have passed through the pores of the steel.

The only alternative left to render wrought iron available for a combustion tube, was to coat it with something that would be impervious to gases. An intimate mixture of borax, lime and finely divided silica, such as is obtained in the preparation of silicofluoric acid, was found to be well suited for a glaze; for when the tube was heated to redness and rolled in a trough containing this mixture, a glaze was imparted to the tube, which proved to be an

effectual preventive to the passage of gases. When a tube thus coated was attached to the pump, and raised, after exhaustion of the air, to a cherry red heat, the mercury did not sink appreciably in the pump; a fact, which showed that a perfectly glazed wrought iron tube is impervious to gases, and also the interesting fact, that if gases were occluded in the iron, they were not disengaged at a red heat, even into a vacuum. It is possible that the iron, by the prolonged and intense heat in the process of glazing, had been so far deprived of its occluded gases, as to yield an inappreciable quantity of them, when afterwards heated in connection with the pump. Be this as it may, when the results observed by heating the tube without glaze, and then with glaze were compared, it seemed in the former case as if the gas passed directly, though less readily, through the iron, as hydrogen is well known to pass through platinum under similar circumstances.

The tube now was in a fit condition for making a nitrogen determination. An analysis of strychnine gave a result agreeing very closely with those recorded in the books, a fact, not only anticipated, but which renders exceedingly probable the feasibility of the method, especially when it shall have been improved by experience and skill. Unfortunately, want of time and other circumstances have prevented an extended series of analyses to test fully the accuracy of the method, as well as a series of experiments to determine the kind and proportion of materials best suited for a glaze. But that such a tube in connection with the Sprengel pump may be used with great advantage in respect to time and expense, there is excellent reason to believe. With such a tube and the Sprengel pump which from its nature is most admirably adapted to working with gases, one is relieved from all anxiety about fracture and the consequent loss of time and labor, the time required for making air-tight connections is reduced to a minimum, and the determination of nitrogen by volume becomes one of the simplest and most expeditious operations of quantitative analysis.

I cannot forbear, before concluding, to call attention to one fact detailed in this paper, as related to another fact, and inference in one of Graham's researches on the porosity of metals for gases. A perfectly glazed wrought iron tube, when heated in a gas furnace, disengages an inappreciable quantity of hydrogen, even into a vacuum. The same tube unglazed, when heated in a similar manner, will allow hydrogen to pass into the vacuum apparently

for an indefinite length of time. If the occluded hydrogen in the iron was expelled by the long and intense heat employed in glazing, how does it happen that the hydrogen found by Graham in meteoric iron, and supposed by him to have come from the celestial spaces, was not expelled by the heat evolved by the passage of the iron through our atmosphere? The thought is suggested whether it be not more reasonable to suppose that the hydrogen found in meteoric iron entered the iron, after being buried in moist earth, and was due to water decomposed by the hot metal, just as in the familiar experiment of obtaining hydrogen by passing steam over red hot iron.

In conclusion, I beg leave to add that the results, detailed in the above paper, were worked out under my direction by Mr. C. P. Jeffers, one of my assistants in the laboratory of the Illinois Industrial University, to whom, for his skill, careful observation and genuine love for chemical research, I am under great obligations.

On Lithium Glass. By Charles B. Dudley, of Philadelphia, Penn.

The alkaline nature of lithium became evident at the moment of its discovery. When, in 1817, Arfvedson found his analysis of petalite to yield a result some four per cent. too low, he was confident, from the method which he had employed, that the loss could arise only from the presence of an alkali in the mineral. And his supposition was verified when, on making another analysis of petalite according to the method for the detection of the alkalies, he avoided the former loss and discovered a new element. In view of the alkaline nature of lithium it occurred to me while working upon lithium and some of its salts during the past winter, in the laboratory of the Sheffield Scientific School, to try whether a glass could be made by means of this alkali. Glass, as is well known, is a compound of silica and an alkali with some base, generally lead or calcium. Thus, common window glass is a silicate

of sodium and calcium; crown glass, a silicate of potassium and calcium; and flint glass, a silicate of lead and potassium. Apprehending that one of the difficulties in the way of making a glass would be the impossibility of obtaining sufficient heat to cause the materials to unite, I determined to employ the most fusible materials, and accordingly tried to make a silicate of lead and lithium. The materials used were silica, minium or red lead, carbonate of lithium, and a very small amount of black oxide of manganese. The silica was obtained from quartz crystals which were finely pulverized and freed from iron by treatment with strong hydrochloric acid. The minium was the commercial article and was found to contain a small amount of iron. The carbonate of lithium was some which I had prepared from the chloride. The oxide of manganese was that commonly used in the laboratory.

The quantities experimented with were, of course, small. In no case were more than twenty-five grams of the several ingredients used at once. The fusing was done in a common wind furnace, the fuel being charcoal and anthracite, only a small amount of the latter, however, being used. Eight fusions in all were made; and of these five were conducted in small Beaufay crucibles, which were exposed to the direct action of the fire; and three, in small porcelain crucibles, which were enclosed in a larger Hessian crucible and this buried in the fire. The amount of materials used varied with the different fusions, but the best piece of glass made contained the following proportions. Silica, 10 grams; minium, 10 grams; carbonate of lithium, 1·5 grams; black oxide of manganese, ·03 gram; cullet from former fusions, 2 grams. The materials were weighed with some care, then well rubbed together in a porcelain mortar, then transferred to the crucible, and thence to the fire. The time of the various fusions varied from one and a half to three hours. At the conclusion of each fusion it was found necessary to break the crucible in order to obtain the glass.

The difficulties experienced in the course of the experiments were as follows:—

1. In the fusions conducted in Beaufay crucibles, so much iron was obtained from the crucible and from the minium as to give the resulting glass a distinct green color.

2. When, to avoid this contamination from the crucible, the fusion was conducted in a porcelain crucible as above described,

sufficient heat could not be obtained to cause the materials to unite completely.

3. Owing to want of sufficient heat, or to errors in manipulation, from none of the fusions was a piece of glass obtained entirely free from striæ or "wreaths."

4. In a laboratory not designed for such work there were, of course, no appliances for gradually cooling and annealing the glass, and consequently the glass, from the various fusions, generally broke up badly on cooling.

Notwithstanding these difficulties, from three of the eight fusions pieces of glass large enough to work with were obtained, and some of their physical and optical properties approximately determined. These may be briefly given as follows:—The glass is clear, transparent and quite hard. It is tough enough, however, so that it may be ground and polished without danger of breaking. It is more or less green in color, but, as those pieces which were known to contain least iron were least green it would seem that the color is due to the presence of iron. This view is strengthened by the fact that those pieces which contain manganese, which substance forms a transparent compound with iron, are likewise least green in color. The index of refraction, of lithium glass, as I have approximately determined it, is 1·6; that of flint glass, as given in Ganot's Physics, is 1·57. An attempt was made to obtain the dispersive power of lithium glass, but this did not prove successful. The specific gravity of lithium glass varies from 3·3 to 3·6. The specific gravity of Guinand's flint glass was the same. That of Fraunhofer had a specific gravity of 3·77. From these considerations it seems fair to conclude, therefore, that a clear transparent glass, of high refractive power, may be made by means of lithium. It remains for future experiments to determine more fully its utility. I ought to add that after I had begun the experiments referred to above, I found that Mr. A. Vernon Harcourt of England had devoted quite a portion of his life to the amateur manufacture of glass, and that in the course of his experiments he had made use of some twenty-nine of the elements, lithium being among the number. A paper giving an abstract of his work may be found in the Report of the British Association for the Advancement of Science for 1871, p. 38, but it tells nothing in reference to the properties of lithium glass.

On the Thermo-electrical Properties of some Minerals and their varieties.[1] By A. Schrauf and Edward S. Dana.

1. All previously published investigations in thermo-electricity have led to this result: that certain minerals are in part *positive*, and in part *negative*, in contact with copper, and that, on this account, they hold different positions in the thermo-electrical series.

If, for example, in the series given by Seebeck,[2] we number bismuth 1 and tellurium 34, we have as

No. 5	Platinum (pure).	No. 7	Copper (pure, from CuO).
No. 14	" (2).	No. 12	" (commercial).
No. 28	" (1).	No. 21	" "

These variations recall to mind the property which belongs to all metals, of suffering very great changes in cohesion, elasticity, etc., in consequence of a minute admixture of some foreign substances.

This important fact has been well established in the case of gold and iron.

The variation from + to —, however, has been shown to characterize crystallized minerals as well as the amorphous metals. Hankel,[3] Marbach,[4] Friedel[5] and G. Rose[6] have investigated this subject in relation to the hemihedral crystals of pyrite and cobaltite. Friedel first suggested the connection between positive and negative thermo-electricity and right and left hemihedrism, but did not pursue the matter further: evidently because, in the case of pyrite, the morphological and electrical relations do not allow of definite determination.[7] Rose attempted to obtain a solution of the same problem by identifying ± with right and left hemihedrism. For the correctness of this supposition, however, there has been as yet no conclusive proof, while, on the other

[1] From the Transactions of the Royal Academy of Sciences at Vienna, vol. lxix, March, 1874.

[2] Seebeck, Gilb. Ann., vol lxxiii, 130; Pogg. Ann., vol. vi.

[3] Hankel, Pogg. Ann., vol. lxii, 197.

[4] Marbach, Compt. Rend., vol. xlv, 707.

[5] Friedel, Institt., 1869, 420; Ann. d. Chim., 1869, vol. xvi, 14.

[6] G. Rose, Pogg. Ann., 1871, vol. cxlii, 13.

[7] In a recent paper (which reached us after the completion of our manuscript) Friedel (Comptes Rendus, xvi, Feb., 1874, p. 508) claims priority for his views over G. Rose, but at the same time he explains that while his hypothesis is to be assumed as true, it does not *crystallographically* admit of proof.

hand, more intricate hypotheses seem to be required for its support.[8] For minerals which do not crystallize hemihedrally, this hypothesis of Rose is evidently inapplicable; and, what is more, if accepted, no variation in electrical character should, in such cases, be expected. It is here important to mention that, some years before the investigations of Rose, exceptions of this nature were observed by Prof. Stefan;[9] he found granular galena negative, crystallized galena positive.

The thermo-electrical series, recently published by Flight,[10] also affords several such examples. His list embraces a large number of minerals. Making hematite 1 (at the negative extremity of the series) and fused chalcocite (No. 3) 56 (at the positive end), we find, as below:

Chalcopyrite	(1)	is	No. 2
Fused chalcopyrite	(1)		21
Galenite			4
Fused galenite			31

What changes, if any, these natural sulphides underwent in consequence of being fused, do not appear to have been accurately investigated, although this is a point of the highest interest. It is clear from the last observations that an explanation is often to be found in an altered condition of aggregation.

The view of Franz,[11] that the direction of the stream ($\pm$) in bismuth depends upon the cleavage lamellæ, offers a satisfactory explanation of some of the phenomena.[12] On the other hand, the right or left hemihedrism entirely fails to account for the change of $\pm$ in holohedral or amorphous fused materials; and in such cases other causes must exist and must be sought for.

2. The thermo-electrical investigation of minerals can have as its object either the determination of their absolute position in the series, or their relative position upon contact with copper. Of these, we selected the latter, since our attention was especially directed to the exceptions, as they may be called. Our method of investigation, consequently, was identical with that which has been

[8] Compare on this subject, Brezina, Tschermak's Min. Mittheil, 1872, p. 23.

[9] Stefan, Sitzungsb. d. k. Akad. Vienna, 1865, vol. li, 260.

[10] Flight, Ann. d. Chem. u. Pharm., vol. cxxxvi.

[11] Franz, Pogg. Ann., vol. lxxxiii, 375; vol. lxxxiv, 388; vol. xcvii, 34.

[12] Schrauf (Lehrbuch d. Phys. Min., II Bd., p. 386) has adopted this explanation. In accordance with the results of the present investigation, this view of the electrical phenomena must be altered.

made use of and fully described by Prof. Stefan and G. Rose. We mention here merely that the homogeneity of our copper wire was carefully ascertained. The wire when warmed and brought in contact with the cold wire left the needle in complete rest,[13] so that no considerable error in consequence of subordinate currents is possible.

A series of preliminary experiments was devoted to establishing the relation between form and thermo-electricity, especial attention being directed to determining the influence of the surface, and of hemihedrism. The following results were obtained:

(*a.*) In galenite, cobaltite and pyrite, the interior of the crystal has the same electrical character as the surface, under normal circumstances; consequently, the particular planes by which the crystals are bounded have no influence upon the $\pm$ sign.

(*b.*) Surfaces made by grinding (on the minerals just named), whatever position they have with reference to the cleavage directions, have the same electrical character with the mass as a whole.

(*c.*) In tetrahedrite and chalcopyrite, which crystallize hemihedrally, and with the most complete opposition of right and left, no change of $\pm$ could be discovered.[14]

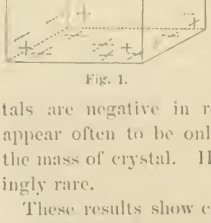


Fig. 1.

(*d.*) On crystals of pyrite the $\pm$ portions are sometimes distributed very irregularly (fig. 1), and any explanation of this by the supposition of a twin structure is impossible. We should have to assume, in this case, a parallel-wise interpenetration of the different individuals or lamellæ.

(*e.*) The majority of the pyrite crystals are negative in relation to copper; the positive portions appear often to be only thin layers, of a different nature from the mass of crystal. Homogeneous + pyrite crystals are exceedingly rare.

These results show clearly that an investigation of the thermo-electrical properties of minerals is of value only when their chemical composition is known. It is well known how much the composition of individual minerals varies; how questionable it

[13] We may mention in addition that a brass wire in contact with itself (cut from the same piece) caused a considerable deflection of the needle, in the direction of the + stream.

[14] Rose (*l. c.*) found the same constancy in the sign in chalcopyrite.

is, for example, whether different pyrites have the same composition.[15] As before observed, a minute admixture of foreign materials may exert a very decided influence upon the applicability of the metals in the arts, and consequently upon their physical characters. On this account our attention, in the following investigations, was directed especially to the chemical side of the subject; and, without meaning to overlook the connection between the form and the variation of ±, we endeavored, first of all, to determine the influence of the material itself. To this end the varieties of certain minerals are especially well adapted; and, in fact, out of a long series of sulphides, arsenides and tellurides of cobalt, iron, nickel and bismuth, we were successful in finding some examples capable of showing the relation between the thermo-electricity and the chemical nature of the minerals. It was necessary first, however, to investigate a large number of species.

3. Our results in regard to the thermo-electrical character of the minerals tested related throughout to their behavior in contact with copper. The position of the wire employed was unfortunately not exactly in the middle of the thermo-electrical series as given by Seebeck (*l. c.*), but on the other hand near pure gold and silver. It was to be expected, in consequence of this, that the larger number of substances would be negative in relation to copper wire; and the observations were in accordance with this. A number of minerals which gave no perceptible stream, and are consequently marked 0 in the following list, might perhaps be + in contact with a different piece of copper. But these facts have only a minor importance in this investigation. Cobalt, nickel and bismuth are strongly negative, and iron strongly positive; their sulphides, on the other hand, act still more vigorously on the needle of the galvanometer than the native metals. The variation from the true result, occasioned by the relative position of the copper wire used, must consequently be quite unimportant.

A. The following minerals gave no electrical stream in contact with copper. Taking into consideration the position of the copper employed by us, as just explained, these minerals must

[15] The use made of pyrite in England (more than eight million cwt. yearly) shows more than any single analysis the amount of Cu, Ag, and Au, contained in this mineral.

stand nearer the positive end of the series (except so far as their non-conducting power was the occasion of the negative result).

Argentite	Ag_2S	isometric.
Acanthite	Ag_2S	orthorhombic.
Sphalerite	ZnS	isomet. hemihedral.
Alabandite	MnS	isometric.
Hauerite	MnS_2	"
Rutile	TiO_2	orthorhombic.
Brookite	TiO_2	monoclinic.
Antimonite	Sb_2S_3	
Boulangerite	$Pb_3Sb_2S_6$	
Kobellite	$Pb_6Bi_2Sb_2S_{12}$	
Sartorite	$PbAs_2S_4$	

This list of minerals investigated embraces unfortunately the majority of those substances from which we expected to throw great light upon our subject.

In the dimorphous group of the silver sulphides the change of form does not correspond to any variation in the thermo-electrical properties. Argentite and acanthite are in this respect similar, so also rutile and brookite. In sphalerite, also, the form seemed to have no influence, although right and left hemihedrism is distinguishable. Alabandite and hauerite were selected in order to show the influence of an increasing percentage in sulphur. The group boulangerite, kobellite, sartorite and antimonite were investigated in order to ascertain their relation to galenite; but no essential differences were observed. It is to be mentioned that (in consequence of their containing antimony) they stand nearer the positive end of the series, while galenite, on the other hand, is strongly negative.

B. The following minerals[16] *are positive* (+), *or negative* (—), *in contact with copper:*

1. *Bismuth compounds.*		[*Bi*—]
Bismuthinite	Bi_2S_3	— Sweden.
Tetradymite	$Bi_2(TeS)_3$	— Schubkau, Orawitza.
Tetradymite	$Bi_2(TeS)_3$	+ Georgia, England.
Wehrlite	$Bi_2(TeS)_4$	+ Hungary.

[16] It is to be observed, in examining this table, that arsenic, antimony and tellurium, as native metals, are + in reference to copper, while in composition they are generally negative.

2. *Nickel compounds.*		[*Ni* —]	
Millerite	Ni S	0.. —	
Gersdoffite	Ni $(AsS)_2$	—	Schladming.
Ullmannite	Ni $(Sb,AsS)_2$	—	Karinthia.
Niccolite	Ni_2As_2	—	Pribram.
Rammelsbergite	$NiAs_2$	—	Hesse.
Grünauite	(NiBiFeCu)S	—	Siegen.
Pyrite (containing Ni)	FeS_2 + 4p.c.Ni	—	Dramen, Norway.

3. *Cobalt compounds.*		[*Co* —]	
Linnæite	Co_3S_4	—	Müsen.
Smaltite	$(CoFeNi)As_2$	—	Saxony, Hesse.
Cobaltite	$Co(SAs)_2$	+ —	Sweden.
Glaucodot	$(CoFe)(SAs)_2$	+ —	Hakansbö.
Alloclasite	$(CoBi_2)_3(SAs_3)_4$	—	Orawitza.
Skutterudite	$CoAs_3$	+	Skutterud, Kongsberg.
"	"	—	Modum, Tunaberg.

4. *Lead compounds.*		[*Pb* —]	
Galenite	PbS	—	Usual varieties.
"	"	+	Monte Poni.
Clausthalite	PbSe	—	Harz.
Naumannite	(PbAg)Se	—	Harz.
Lehrbachite	(PbHg)Se	—	Harz.
Zorgite	(CuPb)Se	+ —	Variable, mixture?

5. *Silver and gold compound.*		[*Ag Au O*]
Sylvanite	$(Ag,Au)Te_2$	—

6. *Copper compounds.*		[*Cu O*]	
Chalcopyrite	$Cu_2Fe_2S_4$	—	
Bornite	$Cu_6Fe_2S_6$	+	
Tetrahedrite	$Cu_4Sb_2S_7$	0.. +	
Chalcocite	Cu_2S	+	
Berzelianite	Cu_2Se	+	Sweden.
Zorgite	(PbCu)Se	+ —	Variable, mixture. Harz

7. *Iron compounds.*		[*Fe* +]	
Marcasite	FeS_2	+	
Pyrite	"	+	Piedmont, Devonshire, Turinsk.
"	"	—	Most localities.

Pyrite (containing Ni)	FeS_2+4p.c. Ni	—	Drammen, Norway.
Pyrrhotite	Fe_7S_8	+	Sweden.
Hematite	Fe_2O_3	—	Brazil.
Magnetite	Fe_3O_4	—	Monroe.
Leucopyrite	$FeAs_2$	—	Andreasberg, Schladming.
Lollingite	Fe_2As_3	—	Reichenstein.
Mispickel	$Fe(SAs)_2$	+	England.
" ?	"	—	Freiberg.
" (Weisserz)	$(FeAg)(SAs)_2$	—	Freiberg.
Danaite	$(CoFe)(SAs)_2$	+	Franconia.
"	"	—	Norway.

Although tables *A* and *B* embrace a considerable number of minerals, they afford only a few general conclusions.

(*a*.) In the compounds of the negative metals Bi, Co, Ni, Pb, the character of the metal outweighs that of the S.

(*b*.) The addition of antimony has the result of weakening this negative character; that of tellurium strengthens it.

(*c*.) In combination with iron the arsenides are negative, but the majority of the sulphides positive.

4. A more accurate understanding of the relations between chemical nature and thermo-electricity can be obtained only from those minerals which are sometimes +, sometimes —. Thus, while in the case of iron, copper, and nickel, the addition of an equivalent of S or As takes place without electrical change, other minerals show a change in sign when the mineralogical formula seems to remain unaltered. With the exception of the observations of Stefan on galenite, those on pyrite and cobaltite are the only other published examples. These latter have been the basis of a very one-sided theory, as it seems to us, although this was natural, since the two species in question are similar in their hemihedral forms.

As a result of our observations, we have succeeded in adding four new cases of minerals with this ± variation. We are inclined to lay especial stress upon these, because these minerals—tetradymite, glaucodot, mispickel, skutterudite—are not hemihedral, so that the change in electrical character cannot be conditioned on right and left hemihedrism.

These cases we regard as proving the dependence of the thermo-electricity upon the chemical composition.[17]

In order to give the proof of this, it is necessary to introduce also into the following tables, besides our observations on the thermo-electricity, the determination of the chemical composition and the density.[18] We did not fail to recognize the desirability of an exact analysis for each mineral whose electrical character was investigated; but in the absence of them we endeavored to supply the want approximately by determining the specific gravity wherever this was possible.[19]

A. *Tetradymite.*

	Schubkau	Orawitza	Georgia	England	Wehrlite Hungary
	+	+	−	−	+
Te	34·6	35·9	48·7		29·7
S	4·8	4·2	0·	?	2·3
Bi	60·0	59·3	51·5		61·1
Fe					
Ag			0·5		2·0
	Wehrle[20]	Frenzel[21]	Balch[22]		Wehrle[23]
G=	7·30		7·868		
	Wehrle[20]		Balch[24]		

The tetradymite discovered in 1873 at Orawitza had the same electrical character as that long known from Schubkau and Deutsch-Pilsen. The composition of these three Te-Bi compounds is in fact similar, and they contain a small amount of sulphur. The variety from Dahlonega, Georgia, is remarkable

[17] Tait has shown recently that iron changes its thermo-electrical sign at a red heat; and this same is true of nickel at a somewhat lower temperature. Tait is inclined to ascribe this phenomenon to a change in the arrangement of the molecules. Our investigations, however, are based upon the character of a stream called out by the minimum amount of heating, thus excluding the possibility of the substance passing into an allotropic condition in consequence of the heat. The molecular arrangement is, therefore, to be assumed as unchanged in these experiments.

[18] Analyses and specific gravity determinations, taken from other authors, are printed in italics.

[19] Each of our specific gravity determinations is the mean of several trials. The average mean error may be stated as ± 0·002.

[20] Wehrle, Schweigg. J., 1830, vol. lix, 482. [21] Frenzel, Leonh. Jahrb., 1873, 800.

[22] Balch, Amer. Jour. Sci., II, vol. xxxv, p. 99.

[23] Wehrle, Baumgart, Zeitsch. Wien, vol. xix, 144. [24] Balch, Dana Min., 1870, p. 31.

for its well-established want of sulphur. In these compounds, as also in sylvanite, the tellurium has an increasing action in a negative direction, while sulphur, notwithstanding the small amount present, acts in a positive direction.

B. *Danaite.*

	Franconia. +	Hakansbö. −	Skutterud. −	Modum. −	Löllingite. $Fe_2 As_3$ Reichenstein. −
G =	6·335	6·096		6·159	
As	41·7		46·7		65·8
S	17·8		17·4		1·8
Fe	32·9		26·2		32·3
Co	6·4		9·1		
	Hayes.[25]		Scheerer.[26]		Karsten.[27]
G =	6·207	6·08			
		6·059			
	Variety Vermontite from Franconia.	Variety Akontite from Hakansbö.[28]			

Besides the Danaite, we have added, for the sake of comparison, the chemical composition of löllingite. Taking into consideration the fact that, in the compounds, arsenic and cobalt play the part of negative elements, but iron and also most of the sulphides that of positive, a partial explanation of the change of ± becomes possible. In the crystals from Franconia the percentage of FeS is larger (= 50·7) than that of AsCo (= 48·1). The reverse is true in the Danaite from Norway, where FeS = 43·6 and CoAs = 55·8. These varieties show a very marked difference in specific gravity.

C. *Skutterudite.*

G =	Modum. − 6·934	Tunaberg. −	Skutterud. + 6·951	Kongsberg. +
As			79·0	
Co			19·5	
Fe			1·4 Wöhler[29]	

[25] Hayes, Am. Jour. Sci., II, vol. xxiv, p. 386.
[26] Scheerer, Pogg. Ann., vol. xlii, p. 546. [27] Karsten, Dana Min., 1870, p. 78.
[28] Dana, Min., 1870, p. 79.
[29] Wöhler, Pogg. Ann., vol. xliii, 591.

The specific gravity of the varieties from Tunaberg and Kongsberg could not be determined, as in the specimens at hand the mineral was in small embedded portions. The variation in the case of the varieties from Modum and Skutterud is especially remarkable. Complete crystals from the Skutterud locality were investigated, as well as massive specimens; they were all +. Skutterudite is isometric, but no hemihedrism has been observed.

D. *Glaucodot.*

	Hakansbö.			Chili.
G=	Shell. — 6·011	Total.	Kernel. + 5·905	?
As	(Fused B. B. to a pearl.)	44·0	(Fused B. B. to a pearl.)	43·2
S		19·8		20·2
Co		16·1		24·7
Fe		19·3		11·0
G=		5·973 Ludwig.[30]		5·975—6·003 Plattner.[31]

The crystals of glaucodot have often a length of 1½ inches. They belong to the orthorhombic system; the cleavage is both basal and prismatic, and may be observed not only in the outer layers of the crystal, but also in the central, although less perfectly. About twenty large crystals from Hakansbö were investigated, all of which showed the same abnormal behavior. The outer shell, 2mm. in thickness, on all the planes, was negative, while the kernel was always positive. If 2mm. of the exterior be filed away, the — shell passes gradually into the + kernel; all fracture surfaces are in the same way +.

In one and the same crystal, then, we find this change of sign. Such a case especially in the orthorhombic system does not allow of hemihedrism being assumed as an explanation. Taking into consideration the great variation in the specific gravity between the exterior and interior portions, as shown by our determinations, it is fair to conclude that the change of ± depends upon some

[30] Ludwig, Sitzungsberichte der k. Akad. Vienna, I, vol. lv, 445, 1867.
[31] Plattner, Pogg. Ann., vol. lxvii, 127, 1849.

change in the composition. What this change was we were not in a position to determine. The blowpipe beads from the outer and inner portions (CoFe) As_2, obtained in the usual manner, were both negative. Their percentage of cobalt was almost the same; one experiment, for example, gave approximately 19·5 per vent. Co. The variations are consequently, in all probability, caused by the elements Fe, S, as in Danaite.

It was a matter of great regret to us that we were unable to obtain for investigation any of the glaucodot from Chili. The analysis of this species is placed in the above table, as the comparison of two analyses shows the possibility of the variation in Fe just mentioned. In the glaucodot from Chili the relation of Co to Fe is 2 :1; in that from Hakansbö 5 :4. It is evident from the specific gravity that the analysis[32] of the glaucodot from Hakansbö was made from a fragment containing both shell and kernel.

An observation of Tschermak[33] adds plausibility to this hypothesis of a variation in the amount of Fe. He describes crystals of cobaltite embedded in the outer portion of a crystal of glaucodot. In the transition from glaucodot to cobaltite, the elements Fe and Co are alone involved, and then only in their relative proportions, thus:

	As	S	Co	Fe	
Glaucodot	44·0	19·8	16·0	19·3	Ludwig l. c.
Cobaltite	43·4	20·8	33·1	3·2	Stromeyer[34]

E. *Galenite.*

	Sardinia granular.	Harz, England.	Pribram crystals.	Kobellite $Pb^4Bi^2Sb^2S^{12}$
	+	−	−	0
G=	7·128		7·575	

The ± varieties are distinguished here, as in all other cases mentioned, by their specific gravity.[35] For the sake of comparison, kobellite has been added: in it, the negative character of the lead

[32] An analysis by v. Kobell agrees completely with the former.

[33] Tschermak, Sitzungsb. d. k. Akad. Vienna, vol. lv, 449, 1867.

[34] Stromeyer., Schweigg. J., vol. xix, 536.

[35] In consideration of the high values of the specific gravity, it is perhaps desirable for us to mention our method of determination. We had at our disposal two balances; a balance made by Kusche in Vienna (maximum load two grams), in the Mineral Cabinet; and another made by Oertling (maximum load 50 grams), private property of Schrauf. We avoided the use of a pygnometer, and adopted in preference the method of two

and bismuth is neutralized by antimony. What part the admixture of Sb, As and Ag play in galenite is uncertain. We note here that we found bismuthinite to be — but boulangerite 0.

F. *Cobaltite.*

Over four hundred and eighty crystals of cobaltite were investigated: of these, the majority were negative, corresponding to the excess of the elements Co and As: only a quarter of them were positive. The crystals themselves are homogeneous and, unlike those of glaucodot, show no difference between shell and kernel. The following are the observations, arranged according to crystalline form.

	No. crystals investigated.	
Cube prominent	49	+
Octahedrons "	242	—
Pyritohedrons "	32	+
" "	20	—
Cube, octahedron and pyritohedron combined	115	—
	24	+

Rose regarded the ± character in this species as dependent upon the right or left hemihedrism. A proof to the contrary, from the consideration of the crystalline form, is not possible, as the proof itself lies in the supposition. It is of more importance, therefore, to ask whether all crystals are of the same composition. All cobalt compounds contain iron to a varying extent. If the other constituents are constant, the specific gravity must increase with the percentage of cobalt; the density of cobalt is greater than that of iron. With this in mind, it is intelligible that for glaucodot we have G=6·0, for cobaltite G=6·3 (mean value, compare analyses above).

This consideration led us to determine the specific gravity of a very large number of the crystals of cobaltite whose electrical

direct weighings in air and water. All our determinations correspond to a mean temperature of 17°-20° C. The agreement of the single observations leaves little to be wished for; we mention some direct results (*not* the mean of several trials) with the weight.

Variety A.	Total 0·64 Gr.,	G=7·430
	6·95 " "	7·425
Variety B.	0·45 " "	7·570
	10·11 " "	7.577

characters had been tested. We give the result in a number of individual cases.

−	+	Exceptions. +
G=6·375 Octahedron	6·072	
6·370 Oct.	5·934	6·411 Cube
6·356 Oct. Tunaberg	6·046	6·415 Cube, Pyr.
6·341 Pyr., Oct.	6·010	Cube alone
6·442 Cube Pyr.	5·927	from Tunaberg
6·387 Oct. Pyr.	5·905	
	6·151	
	6·160	
	6·215	Pyritohedrons from
	6·263	Hakansbö
	6·208	Pyr. from
	6·265	Tunaberg
	5.984	Pyr. from Skutterud.

These figures agree essentially—to 80 or 90 per cent.—with our preceding conclusions, that the crystals rich in cobalt are negative and have a higher specific gravity. We may, however, with more certainty conclude that the octahedrons are negative and have $G>6{\cdot}30$, while the cubes are positive and have $G<6.1$; the pyritohedrons vary in sign $\pm$, and have $G>6{\cdot}1$. We found also two exceptions in the density, which we place beside the others without attempting an explanation by the suggestion of a possible admixture of nickel.

G. *Sulphides of Iron.*

The important work of G. Rose has directed especial attention to this species. On page 68 we have already given some results, which we obtained under the supposition of some essential connection between form and electrical character. A few observations are here added which relate especially to the chemical composition.

Sulphide of Iron, artificial.	Marcasite.	Pyrites.		
+	+	−		+
Flight[36]	G = 4.83	G=5·019	Elba	G=4·866 Cubes
		=5·020	Piedmont	=4·941 Devon.
		=5·195	*polished crystals.*	=4·992 Cube
			Zephar.[37]	=4·998 Turinsk

[36] Flight, Ann. Chem. Pharm., vol. cxxxvi.

[37] Zepharovich, Dana Min., 1870, p. 63.

Among the very large number of crystals we were able to investigate, we found only a few which over the whole surface as well as in the interior, were homogeneous +[38]. This will explain the small number of determinations of specific gravity.

The abnormally low density of the cubes from Devonshire suggests that they may be altered material, pseudomorphs of marcasite after pyrite. The mineralogical habit is still that of pyrite, but the polish of the planes is gone. Some + spots upon other— crystals would perhaps have a similar explanation.

The higher specific gravity of pyrite in comparison with marcasite, and its —character (marcasite +), and its crystalline form resembling that of cobaltite, may possibly all have the same cause, viz: the admixture of Cu, Ni, Ag, Au, which gives pyrite its metallurgical value. The ± varieties of FeS_2 are also distinguished by their density. We do not venture to draw any further conclusions from these scanty observations.

5. By the preceding investigations, the fact has been established that all ± varieties of minerals are distinguished also by their specific gravity. It is remarkable that for tetradymite, glaucodot, skutterudite, galenite, cobaltite and pyrite the density of the positive variety is less than that of the negative variety. We simply mention this fact without wishing to establish any dependence of the thermo-electricity upon the density in general, the specific gravity having been taken by us as merely an indirect method of arriving at the chemical composition. The influence of the form upon the density we have attempted to exclude by making our comparisons only with varieties having the same structure, comparing crystals with crystals, massive specimens with massive. The effect of the temperature[39] was also eliminated so far as possible.

With reference to the influence of the form upon the thermo-electricity we may remark in conclusion: The investigations of alloys of SSe, of BiS, of the oligoclase feldspars, of $(KaNa)_2\ SO_4$ has shown that the crystalline form does not change with the composition by regular gradations, but rather by abrupt leaps, and consequently remains identical within certain limits of chemical variation. The density, optical properties, and, as we regard it,

[38] The majority of the crystals of pyrite are negative.

[39] On the influence of temperature, compare note 1, p. 73. It may be mentioned in addition, that a change in density must also accompany the change of ± in the case of iron at an elevated temperature.

thermo-electricity give a sharper indication as to identity or difference of material, than the crystalline form. Before establishing relations between the form and thermo-electricity, it must first be shown that the material in hand is identical.

Our observations have demonstrated that in some cases the change in thermo-electrical character corresponds to a change in the chemical composition, and always to an alteration in the density.

On the Nitrogen of the Soil. By J. P. Armsby, of Millbury, Mass.

That all plants require, for their complete development, a supply of nitrogen, is one of the best known facts of vegetable physiology; and this fact being known, it became one of the most important problems of agricultural chemistry to find the source whence the plant derives its supplies of this indispensable element.

The most obvious source is the atmosphere, containing four-fifths its volume of nitrogen; and although it exists there in the free state, and is known to be a comparatively inert element, it has been thought that plants, by virtue of their so-called "vital force," might be able to assimilate it.

It has, however, been proved by the elaborate investigations of Lawes, Gilbert, and Pugh (Phil. Trans. 1861, II, pp. 431–579), that the *cereals*, at least, are incapable of assimilating free nitrogen, and the same is probably true of all plants.

It is improbable that the compounds of nitrogen (nitric and nitrous acids, and ammonia) existing in the atmosphere are assimilated to any appreciable extent *from the air*, under natural conditions; and it has been abundantly proved that a fertile soil is capable of supplying vegetation with all needful nitrogen: and it probably does so.

All circumstances, then, which affect the supply of this element in the soil are of the highest interest to the agriculturist.

Upon this point two classes of facts are known;

1st. It has been shown that nitrogenous organic substances, such

as exist in freshly manured soils, may yield nitrogen in the free state by decomposition; though the manner in which this takes place, and the circumstances favoring it, are imperfectly known as yet.

2d. It has been shown that under some circumstances, organic bodies, such as exist in the soil, are capable of causing free nitrogen to enter into combination in them, and thus of increasing their content of that element. This increase has been generally attributed to the formation of nitric acid from free nitrogen, by some oxidizing action.

Among the early experiments on the gain of nitrogen by the soil are those of Mulder (Chem. of Animal and Vegetable Physiology, p. 673), who grew bean-plants in a soil composed in one case of ulmic acid, and in another of charcoal, both free from nitrogen. The plants were supplied with no nitrogen except the free nitrogen of the air.

On comparing an analysis of the crop with that of a sample of the seed, it was found that the crop contained from two to three times as much nitrogen as the seed.

Since free nitrogen is not assimilated by plants, the gain of nitrogen must have taken place through the medium of the soil.

Mulder believed that the gain was due to the formation of ammonia from free nitrogen, and nascent hydrogen set free by decomposition; but it has been shown by Will (Ann. Chem. Pharm. 45, 106) that this reaction does not take place.

In 1858 & 59, Boussingault (Agronomie, Chemie Agricole et Physiologie, T. 1, p. 318) executed some experiments on this subject.

In his first experiment he spread out thinly 120 grammes of the very fertile soil of his garden in a glass dish, and for three months moistened it daily with water free from compounds of nitrogen. At the end of that time it had lost ·825 per ct. of carbon, and gained ·008 per ct. of nitrogen.

In a series of five experiments, in which lupins, hemp and beans were grown, either in free air but sheltered from rain and dew, or in a confined volume of air, in small quantities of the same soil mixed with sand, the plants gained nitrogen in every case, and the soil in all but one, in which the gain by the plant was unusually large and doubtless came from the soil.

The gain of nitrogen was greatest in two cases in which a confined volume of air was used, showing that it was not due to the absorption of nitrogen compounds from the air.

The detailed results of one experiment are subjoined.

Weight of soil 130 gms. Weight of seed (Lupin) ·384.

	Before expt.	After expt.	Dif.	Dif. expressed as N.
N. of seed (or plant) .	·020	·0417	·0217	·0217
N. of soil	·338	·3834	·0454	·0454
NH_3 of soil	·00281	·00567	·00286	·00235
N_2O_5 of soil	·00044	·01172	·01128	·00395

The gain appears in part as nitric acid, and in part as ammonia, the increase of nitrogen in the form of nitric acid being the greater; but the total increase of nitrogen *is much greater than the sum of these two.*

In an experiment exactly similar, except that no vegetation was introduced, the gain of nitric acid was much greater.

In similar experiments on a soil destitute of organic matter, no nitrogen accumulated.

This would seem to show that the presence of organic matter is necessary for the fixation of nitrogen; but the experiments of Clocz, on nitrification, shortly to be described, show that a gain of nitrogen may take place in the absence of organic matter.

Boussingault has also made some later experiments (Compt. Rend. 76, 22) which are of interest. Portions of a rich vegetable soil, mixed with three or four times their weight of sand, were placed in large glass vessels holding nearly one hundred litres, which were sealed tightly and left in a cellar for eleven years.

The amount of nitric acid increased over two hundred-fold; but the total nitrogen *decreased* slightly.

It is thus shown that nitrification is not always accompanied by an increase of combined nitrogen; but that a vigorous nitrification may take place at the expense of the organic matter of the soil.

As has already been stated, the gain of nitrogen observed above has been attributed to the production of nitric acid; and several theories have been proposed to account for its formation.

Schönbein believed that water, during evaporation in the air, unites with nitrogen, forming nitrite of ammonia: but Zabelin (Ann. Chem. Pharm. 130, 76) has shown that simple evaporation of water in the air will not produce nitrite of ammonia; though in the presence of cellulose, or some similar organic body, it is formed in abundance. Another theory is that free nitrogen is oxi-

dized by some body capable of alternate oxidation and reduction, like sesquioxide of iron, in the same way in which ammonia is oxidized with the formation of nitric acid.

Cloez, in a carefully conducted experiment (Leçons de Chemie Professees en 1861 á la Société Chemique de Paris, p. 145), caused a continual stream of purified air to pass, for eight months, through ten large vessels containing various porous materials.

No. 1 contained fragments of unglazed porcelain; No. 2, calcined pumice-stone; No. 3, fragments of washed brick. These materials were drenched with a solution of ten grammes of carbonate of potash in water.

The next three vessels (Nos. 4, 5, & 6) contained the same porous materials, in the same order; but the carbonate of potash was replaced by carbonate of lime suspended in water.

No. 7 contained washed and dried chalk. No. 8 contained a clayey soil, washed and ignited; No. 9, the same soil, washed and dried, but not ignited. No. 10 contained moist pumice-stone, mixed with pure carbonate of lime and ten grammes of urea. During the experiment 160,000 litres of air were passed through the apparatus. At the close of the experiment No. 1 contained traces of nitric acid which probably came from the air, having escaped the purifying apparatus. Besides this, nitric acid was found in considerable quantities in Nos. 3 and 6, containing fragments of brick, and in No. 9, containing the washed soil. Traces of nitric acid were found in the chalk and the ignited soil. The others were free from it.

The absence of nitric acid in Nos. 2, 4, and 5, shows that the concourse of air, a porous body, and a carbonated alkali, is insufficient for the production of nitric acid.

It is evident that some substance was present in the brick fragments which determined the oxidation of free nitrogen, since the materials used were free from compounds of nitrogen. Cloez believes that the nitrification observed in these two cases was due to the presence, in the brick, of some oxidizable mineral matter, probably sulphide of iron, which acted as a "carrier of oxygen."

The abundance of nitric acid in the washed soil is in striking contrast with the mere traces found in the ignited soil, and makes evident the influence of humus. Whether this nitric acid was due to the oxidation of free nitrogen, or of the combined nitrogen of the soil, cannot be determined from Cloez' experiments: though

the absence of nitric acid in No. 10, containing the highly nitrogeneous body, urea, diffused through a porous medium, and in contact with a base, is very interesting in its bearings on this point.

A third way of accounting for the fixation of nitrogen by the soil is the supposition that the nitrogen contained in the interstices of the soil is converted into nitric acid by *ozone*, produced by the slow oxidation of the humus of the soil, or possibly by electrical disturbances. This theory seems very probable, and is supported by numerous facts. It is well known that in many, if not all, processes of slow oxidation, ozone is formed, and also that ozone is capable of oxidizing free nitrogen to nitric acid in the presence of water. Gorup-Besanez (Ann. Chem. Pharm. 161, 232) has shown that during the evaporation of water in contact with organic matter, ozone is formed; and this fact, which is probably the explanation of the results of Schönbein and of Zabelin, already mentioned, goes to confirm the theory just stated.

The condensation of nitrogen and oxygen in the pores of humus, which has been shown to take place by Reichardt and Blumtritt (Journ. für Prackt. Chem. 98, 476), would also favor this reaction.

Lawes, Gilbert, and Pugh (Phil. Trans. 1861, II, 495) have indeed shown that, under some conditions of decay, ozone does not produce nitrification. They caused strongly ozonized air to pass, for six months, over moist mixtures of ignited soil with starch, sawdust, and bean-meal, with and without lime: also over starch, sawdust, and bean-meal, moistened with water; and over ordinary garden-soil. At the end of the experiment, no nitric acid was found except in the garden-soil. This was found to have contained it originally, and no estimation was made of the gain, if any.

These results, however, can only be taken as showing that organic matter must have passed into the comparatively stable condition of humus before ozone will produce nitrification.

It is highly probable that a fixation of free nitrogen by the soil does take place in one, or perhaps in both these ways; but the results of some recent investigations render it probable that nitrification is not the only method by which organic matter may fix free nitrogen.

Dehérain, in a recent investigation (Annales des Sci. Nat., Botanique, Séries V, 18, 147), has obtained some interesting results

in regard to the fixation of nitrogen by organic bodies in the presence of an alkali. He experimented on glucose, humus, sawdust, decayed wood, and the "azotized glucose"[1] of Thenard, mixed with potash, ammonia, and lime, by heating them on the water bath, in a sealed tube containing a known volume of air, and measuring the amount of nitrogen and oxygen which disappeared. He made sixty experiments, with various mixtures, and obtained in twelve cases an absorption of nitrogen, and in several cases *more disappeared than was sufficient to have formed nitric or nitrous acid with all the oxygen present.*

In other cases there was an evolution of free nitrogen by the contents of the tube. The absorption of nitrogen was greatest in two cases in which all the oxygen was consumed.

A second series of experiments was made on a known volume of air, over mercury, in the cold, with the same substances used in the first series. The results were similar to those of the first series in every respect, except that they were smaller in amount.

In a third series of experiments, pure nitrogen was passed through gently heated solutions of pure glucose and alkali, contained in a tightly closed glass globe.

In every case, the residue left on evaporating the solution to dryness contained a considerable quantity of combined nitrogen.

In one case ten grms. of glucose fixed ·071 grms. of nitrogen, in another case ·065 grms. On substituting air for nitrogen in the latter experiment, the gain fell to ·015 grms., indicating that oxygen hinders the reaction. In a fourth series of experiments, pure nitrogen was substituted for the air of the second series. Twenty-one experiments were made, and of these, nineteen showed a fixation of nitrogen during the experiment; but the results are singularly variable; *e.g.*, 3·1 c. c. and 5·9 c. c. for duplicate experiments on glucose and soda. The most decided results were obtained with mixtures of glucose and potash or soda. No examination was made of the solid materials after the experiments.

Though the methods of experiment adopted in this extensive investigation seem open to criticism in some respects, the general results are very interesting, and undoubtedly show a fixation of nitrogen by organic matter in the presence of, at most, an extremely minute quantity of oxygen; certainly of far less than was

[1] Prepared by passing ammonia into a solution of glucose.

requisite to convert the amount of nitrogen assimilated, into nitric or nitrous acids.

In regard to the escape of nitrogen in the free state during the decomposition of nitrogeneous organic matter, experiments, though less numerous, are equally conclusive as to the fact of its occurrence.

Reiset (Recherches Expérimentales et Pratiques sur l'agronomie, 1865, p. 48) has proved the escape of nitrogen in the free state, from fermenting dung.

Lawes, Gilbert, and Pugh (Phil. Trans. 1865, II, p. 501) have made a large number of experiments on this subject, and their results fully confirm those of Reiset. In fifteen experiments, in which wheat, barley, and beans, both entire and in the form of meal, mixed with ignited soil or with pumice, and with varying quantities of water, were allowed to decay in pure air, a loss of nitrogen, in some form other than ammonia, of from 1·74 per cent. to 40·2 per cent. of the quantity used, took place in thirteen experiments, while in two there was a slight gain.

Nothing was observable in the conditions of the experiments to which these two exceptional results could be attributed.

No nitric acid was found in any case at the close of the experiments. Considerable ammonia was found, but no connection could be observed between its amount and the loss of nitrogen.

The same investigators have shown that in the absence of oxygen, this loss of nitrogen does not take place.

They caused vegetable matter to decay in water, over mercury, and out of contact with the air. Large quantities of gas were given off, but in every case it consisted of carbonic acid (CO_2) and hydrogen, with no nitrogen. This shows plainly that the loss of nitrogen is not due to a reducing action, since the action of nascent hydrogen failed to liberate it. These authors consider, with good reason, that the loss is due to an oxidation of amide-like bodies, by which the hydrogen is united to oxygen, and nitrogen is set free. A similar reaction is known to take place with some definite amides, and it is very probable that this explanation is the correct one.

Schloesing (Compt. Rend., 77, 353) has studied the reducing action of the soil in the absence of oxygen; 11·5 kilos. of a fertile, calcareous soil, mixed with 7·5 grms. of nitrate of potash, being used for the experiment. Free nitrogen was given off from

the mixture, exceeding by 196·7 c. c. that contained in the nitrate of potash introduced. This excess must have been derived from the soil, and the author believes that it was set free by the oxidizing action of the nitrate of potash on the amide-like bodies, already referred to, during the process of reduction.

König and Kiesow, in some experiments on the loss of nitrogen (Central Blatt für Agrikultur chem. II, 7, 9), have succeeded in preventing it by the addition of gypsum to the materials used.

The following experiments were undertaken in the hope of throwing some light on the conditions favoring the gain of nitrogen on the one hand, and its loss on the other.

The method adopted was to allow organic matter containing a known amount of nitrogen to decay under such conditions that all the ammonia given off could be collected and estimated, and at the end of the experiment to determine again the nitrogen and ammonia.

Two sets of experiments, of four each, were made, the first set (I) being conducted in carefully purified air, the second (II) in nitrogen prepared from air by means of phosphorus, and carefully purified, but still containing traces of oxygen.

With the exception of the atmosphere, the conditions of corresponding experiments in the two sets were the same.

The organic matter used consisted of dried and sifted barnyard manure, intimately mixed with about one-fourth its weight of dried and pulverized flesh. The mixture contained 2·11 per ct. of water.

The following were the materials and quantities used in each experiment.

		Organic matter	Gypsum.	Potash (KOH).	Water.	Total Nitrogen.
In air.	I. 1.	15 grms.			6 c. c.	0·486 grms.
	I. 2.	15 "		0·798 grms.	6 c. c.	0·486 "
	I. 3.	15 "	15 grms.		6 c. c.	0·486 "
	I. 4.	15 "	15 "	0·798 "	6 c. c.	0·486 "
In nitrogen.	II. 1.	15 "			6 c. c.	0·453 "
	II. 2.	15 "		0·798 "	6 c. c.	0·453 "
	II. 3.	15 "	15 "		6 c. c.	0·453 "
	II. 4.	15 "	15 "	0·798 "	6 c. c.	0·453 "

The potash was from a standard solution of which 7 c. c. were employed in each experiment, equivalent to about 6 c. c. of water.

The mixtures were made moist but not coherent. All the inorganic materials used were free from combined nitrogen. The first set was begun Mar. 9, 1874, the second Mar. 31, 1874, the per cent. of nitrogen having decreased slightly in the interval, owing to an absorption of water.

Purified air, amounting in all to about 5800 c. c. for each experiment, was aspirated daily through I, during the time of the experiments.

The temperature averaged about 70° F. varying from 50°–80° F.

The apparatus stood in diffused daylight, but not in direct sunlight.

The experiments terminated May 15–21, 1874, when the mixtures were dried with suitable precautions, and analyzed for nitrogen and ammonia.

The following are the results calculated on the whole quantity used. [See table on opposite page.]

By an inspection of this table we see;

1st. That with the exception of I, 2, and II, 2, there is a loss of nitrogen in every case (the slight gain in II, 1, being within the errors of experiment).

2nd. That this loss is very much less in the second set of experiments, where only traces of oxygen were present; and this is not due to the shorter duration of the experiments entirely, since the *gain* observed was greater in II, 2, than in I, 2, the corresponding experiment of series I. This result agrees with those of Lawes, Gilbert, and Pugh, already described, and goes to show that the loss of nitrogen is caused by a process of oxidation. The effect of the gypsum seems to be to prevent, in part, the loss of nitrogen. There seems to be no obvious relation between the circumstances of the experiments and the amount of ammonia formed.

3rd. We see that in the two experiments in which the organic matter was mixed with potash, a considerable gain of nitrogen took place, which was the greater in the experiment in which only traces of oxygen were supplied.

These two mixtures were carefully tested for nitric acid, but no trace of it was found; thus showing, either that none had been formed, or that if formed it had afterward been decomposed, and had given rise to some other compound of nitrogen.

	SUBSTANCES USED.	TOTAL NITROGEN.				AMMONIA EXPRESSED AS NITROGEN.		
	Description.	Before Decomposition	After Decomposition	Gain (+) or Loss (—)		Before Decomposition	After Decomposition	Gain.
				Weight.	Per cent.			
I.1.	Organic matter.	0·486 grms.	0·432 grms.	—0·054 grms.	—11·11	Trace.	0·0118 grms.	0·0118 grms.
I.2.	Organic matter, and Potash.	0·486 "	0·560 "	+0·074 "	+15·22	"	0·0301 "	0·0301 "
I.3.	Organic matter, and Gypsum.	0·486 "	0·4558 "	—0·0302 "	— 6·21	"	0·1651 "	0·1651 "
I.4.	Organic matter, Potash and Gypsum.	0·486 "	0·4224 "	—0·0636 "	—13·09	"	0·0907 "	0·0907 "
II.1.	Organic matter.	0·453 "	0·4597 "	+0·0067 "	+ 1·48	"	0 0614 "	0·0614 "
II.2.	Organic matter and Potash.	0·453 "	0·5406 "	+0·0876 "	+19·34	"	0·0255 "	0·0255 "
II.3.	Organic matter and Gypsum.	0·453 "	0·4478 "	—0·0052 "	— 1·14	"	0·0784 "	0·0784 "
II.4.	Organic matter, Potash and Gypsum.	0·453 "	0·4442 "	—0·0088 "	— 1·94	"	0·0479 "	0·0479 "

That nitric acid may be reduced to ammonia in the soil, and that this ammonia may unite with the humus bodies of the soil to form compounds in which ammonia is no longer recognizable, is doubtless true; but that such an action took place in these experiments cannot be admitted.

In the first place, it is evident from the table that the nitrogen gained does not *all* exist as ammonia; and it is most probable that all the ammonia found was produced by the decomposition of the organic matter, since the amount found was greatest in cases in which there was a loss of nitrogen, and in which it apparently must have been formed in this way.

In the second place, the gain of nitrogen was the greater in the experiment in which, at most, only traces of oxygen were present, and which continued for the shorter time; while Schloesing (Compt. Rend. 77, 203) has shown that nitrification goes on with greater rapidity in the presence of an increased proportion of oxygen. We must, then, conclude that the gain of nitrogen observed in these experiments was not due to nitrification; and this result agrees so remarkably with those of Deherain, both in regard to the materials used, and the greater gain of nitrogen in the absence of oxygen, that we may consider it as at least very probable that *organic substances in the presence of caustic alkalies may fix free nitrogen without the formation of nitric acid.*

That the gain cannot be due to the direct formation of ammonia has already been stated.

In what way such a fixation can take place, we have little ground for conjecture, but it appears to be a fact.

As the general result of the foregoing discussion, then, we reach the following conclusions, which we may regard as expressing the present state of our knowledge on the subject.

1st. That the loss of nitrogen in the free state during the decomposition of nitrogeneous organic matter, itself a well established fact, is due generally to an oxidizing action.

2nd. That an increase of the combined nitrogen of the soil may take place by an oxidation of free nitrogen to nitric acid.

3rd. That some organic substances, in the presence of a caustic alkali, are able to fix free nitrogen without the agency of oxygen, or the formation of nitric acid.

On the Periodicity of the Rainfall in the United States, in relation to the Periodicity of the Solar Spots. By John Brocklesby, of Hartford, Ct.

The researches of scientists, especially of late, lead to the conclusion that there is an intimate connection, more or less marked, between the solar disturbances and various terrestrial phenomena. Thus, upon comparing the mean daily range of the magnetic declination, and also the number of auroras observed each year, with the extent of the spots on the solar disk, a striking correspondence is observed in the curves which respectively represent these phenomena. A periodicity in the cyclones of the Indian Ocean and West Indies, corresponding with that of the sun-spot area, is asserted by Mr. J. N. Lockyer to have been established by Mr. C. Meldrum, Director of the Meteorological Observatory at Mauritius, and it is also claimed by Mr. Meldrum and others, that the variation in the rainfall conforms also to the sun-spot cycle of about eleven years. Extended investigations likewise point to the inference that changes in the annual temperature of the atmosphere occur in cycles of ten or eleven years, coincident with those of the solar commotions. It is proposed in this paper to discuss the connection between the variations in the annual rainfall and the variations in the extent of the solar spots, as regards this continent, so far as we have data for the same. At the meeting of the British Association last year, Mr. Meldrum, in a paper on the "Periodicity of Cyclones and Rainfall in connection with the Sun-spot Periodicity," remarked as follows: "With the help of the researches of Mr. Lockyer, Mr. Symons and Dr. Jelinek of Vienna, I have now examined ninety-three tables of rainfall for various parts of the world, and I find that, scarcely without exception, more rain falls in maxima than in minima sun-spot years. So far as observations go, Europe, Africa, America and Australia give very favorable results. Asia is represented, however, only by three stations, and France, which affords as yet only five stations, is the only European country which presents results opposed to the theory."

From the comparatively small number of tables of rainfall which Mr. Meldrum gives, and from his silence upon the subject, we may I think, safely conclude, that he did not consult Mr. Charles Schott's elaborate article on the rainfall in the United

States, published by the Smithsonian Institution; a work which embraces abstracts of records of aqueous precipitation from about twenty-two hundred stations. The investigations therefore of Mr. Meldrum, so far as this country is concerned, may be regarded as incomplete.

As the following discussion is based upon Mr. Schott's tables, it may not be amiss to state briefly in what manner they have been so constructed and arranged that the variations in the annual rainfall throughout the United States admit of ready comparison with the changes in the extent of the solar spots.

From the whole number of stations whose mean annual rainfalls are respectively recorded, Mr. Schott selects one hundred and seven, which, on account of their extent of record, involve the smallest probable errors in their average rainfalls; and he then ascertains the ratio of each annual rainfall to the average yearly rainfall, for every one of these stations. The period of record of these stations extends, with greater or less intervals, from 1799 to 1867.

To free these ratios from accidental irregularities, and to exhibit the nature of the fluctuations from year to year more distinctly, the author unites them in groups, formed of stations where the annual rainfall appears subject to the same laws. Group I is composed of stations on the Atlantic sea-board extending from Maine to Virginia. Group II comprises the State of New York and adjacent parts of Canada, New Hampshire, Massachusetts, and Vermont. Group III includes parts of Iowa, Minnesota, Illinois and Wisconsin. Group IV embraces the Ohio Valley, Ohio, Illinois, Indiana, Kentucky and parts of Missouri. Group V is formed of the Indian Territory and Arkansas. Group VI comprises Louisiana, Alabama and West Florida. Group VII comprehends the seacoast from Virginia to Florida, and Group VIII, includes the seacoast of California.

In these groups the percentage of the mean amount of rainfall is tabulated for each year of observation; the longest period of record extending from 1804 to 1867.

From the data thus afforded curves are constructed, which present to the eye the annual fluctuations of the rainfall over the vast region embraced in these groups. Mr. Schott speaks briefly of the connection between the solar disturbances and the rainfall, stating that the rain curve for 1837–8 shows a decided minimum

in precipitation, when there was a marked maximum of solar activity; but that the two phenomena lead to an opposite conclusion about the epoch of 1855–6, a minimum of rainfall then occurring with a minimum extent of sun-spot area.

He does not enter into any detailed investigation of this supposed connection.

Under these circumstances it appears therefore desirable, in order to detect what connection, if any, exists between the fluctuations of the annual rainfall and the variations in the extent of the solar spots, that these phenomena should be compared either year by year, or by groups of years, and this it is now proposed to do.

Taking Dr. Wolf's table of the relative extent of sun-spots for each year, within the period from 1804 to 1867 inclusive, in which period the yearly percentage of the average rainfall is also given in Mr. Schott's table of territorial groups, two methods of comparison can be employed if we wish to ascertain whether an annual excess of sun-spot area is attended by an excess of annual rainfall and *vice versa*. The first mode is to take the average of the numbers representing the relative extent of the spots for the period mentioned, viz., from 1804 to 1867, then to note the years in this period that are above or below the average, and next to place the percentage of the mean annual rainfall opposite each year. We can thus detect whether an excess of sun-spot area in any year is marked by an excess of rain, and a deficiency of the former by a deficiency of the latter, or whether no such law exists. The second method is that adopted by Mr. Meldrum, who makes the comparison by forming quinquennial sets, takes two years on each side of the maximum year, for a maximum set, and two on each side of the minimum year, for a minimum set, or where this is not possible, a triennial group is formed.

Proceeding by the first method, it is found that the average sun-spot area, for the period extending from 1804 to 1867 inclusive, is denoted by the number 38. Taking now each year of this period, the year is printed in full-faced or light-faced type, according as the number representing its sun-spot area is greater or less than 38, full-faced type indicating an excess, and light-faced type a deficiency. Each year is then also marked plus or minus from Mr. Schott's table of territorial groups, according as its rainfall is above or below the mean.

This being done, we have the following results. In group I,

comprising the seaboard from Maine to Virginia there are twenty-nine years in which the sun-spot area is above the average. In seventeen of these the rainfall is in excess, in ten below the average, and in two equal to it. In this group there are also thirty-five years when the sun-spot area is below the average; out of these there are sixteen years when the rainfall is in excess, and nineteen years when it is below the mean.

In group II, comprising the state of New York and adjacent parts of Canada, New Hampshire, Massachusetts and Vermont there are twenty-five maxima sun-spot years. In fifteen of these the annual rainfall is in excess, in nine below the mean and one when it is equal to it. There are also in this group seventeen minima sun-spot years, and in these the rainfall is in excess seven times, below the mean eight times, and equal to it twice.

In group IV, a region embracing Ohio valley, Ohio, Indiana, Illinois, Kentucky and part of Missouri, there are twenty-five years of excess of sun-spot area, and in thirteen of these the rainfall is in excess of the mean, in twelve below it, and in one equal to it. There are also in this group, twenty-five years when the extent of sun-spots is below the average, and in nine of these the rainfall is above the mean, in fourteen below the mean, and in two equal to it. The tables of precipitation belonging to these three groups, are regarded by Mr. Schott as tolerably trustworthy, and the results of this comparison show a tendency, on the whole, towards an excess of rain when there is an excess of sun-spot area, and *vice versa*. Yet we meet here with marked anomalies, for in the period from 1818 to 1826 inclusive, which are years of deficiency in solar activity, six annual rainfalls out of the nine are above the mean; and from 1835 to 1840 inclusive, which are years of excess in the sun-spot area, all the annual rainfalls in the three groups are below the average, except in one instance. Moreover, years of excess and deficiency of rainfall are found as well in the periods when the sun-spot area is above the mean, as in those where it is below it.

Passing now to the other territorial groups, the results from which are to be regarded as only rough approximations towards the truth, on account of the insufficiency of the stations, we obtain the following results from all these taken together. In the years when the extent of the spots is in excess, the annual rainfall is above the mean thirty-five times, below it thirty-seven

times, and equal to it five times. In the years when the sun-spot area is below the mean, the rainfall is below the average thirty-two times, above it twenty times and equal to it six times.

Taking now all the groups which embrace so large a portion of the United States, we deduce the following results in reference to the subject before us:

	Sun-spot area.	Ratio of the excess of rainfall (above the mean) to its deficiency.
From 1804 to 1805 (inclusive)	above the mean	0·1
" 1806 to 1815	below "	6·4
" 1816 to 1817	above "	0·2
" 1818 to 1826	below "	6·12
" 1827 to 1831	above "	12·4
" 1832 to 1834	below "	5·7
" 1835 to 1840	above "	6·29
" 1841 to 1845	below "	12·19
" 1846 to 1852	above "	31·18
" 1853 to 1857	below "	8·28
" 1858 to 1864	above "	31·15
" 1865 to 1867	below "	15·3

For the reasons already stated, namely, that groups I, II and IV are only to be regarded as trustworthy, and that the rest give but rough approximations, the above results must be taken with great allowance. As they stand they are full of anomalies, about half the results favoring and the rest contradicting the law which is supposed to exist between the rainfall and the extent of the sun-spot area.

The results obtained by this first method of comparison are exhibited in the annexed table: (Table I).

The second mode of investigating any relation that may exist between the annual fluctuations in the extent of the solar spots and the variations in the yearly rainfall, consist in forming groups of years, by taking one or two years on each side of the maximum and minimum year of any sun-spot period. As the mean secular period of the spots is about eleven years, quinquennial

TABLE I.

Group.	I. Seacoast Maine to Virginia.	II. State of N.Y. & adjacent parts of Can., N.H., Mass., & Vt.	III. Parts of Ia., Minn., Ill., & Wis.	IV. O. Valley, O., Ind., Ky., Ill., & pt. of Mo.	V. Indian Territory & Arkansas.	VI. Louisiana, Ala., & W. Florida.	VII. Seacoast of Virginia to Florida.	VIII. Seacoast of California.	+ Rainfall above the mean. — below it. = equal to it.		Total. +	Total. —
1804	=	..	..	..	..	..	..	..		0	..	..
1805 Max.	—	..	..	..	..	..	..	..		1	..	−1
1806	—	..	..	..	..	..	..	..		1	..	..
1807	+	..	..	..	..	..	..	..	1		..	..
1808	+	..	..	..	..	..	..	..	1		..	..
1809	+	..	..	..	..	..	..	..	1		..	..
1810 Min.	—	..	..	..	..	..	..	..		1	..	..
1811	—	..	..	..	..	..	..	..		1	..	..
1812	+	..	..	..	..	..	..	..	1		..	..
1813	+	..	..	..	..	..	..	..	1		..	..
1814	+	..	..	..	..	..	..	..	1		..	..
1815	—	..	..	..	..	..	..	..		1	+ 6	−4
1816 Max.	—	..	..	..	..	..	..	..		1	..	..
1817	—	..	..	..	..	..	..	..		1	..	−2
1818	—	..	..	+	..	..	..	..	1	1	..	..
1819	—	..	..	—	..	..	..	..		2	..	..
1820	—	..	..	—	..	..	..	..		2	..	..
1821	—	..	..	=	..	..	..	..		1	..	..
1822	—	..	..	+	..	..	..	..	1	1	..	..
1823 Min.	—	..	..	+	..	..	..	..	1	1	..	..
1824	—	..	..	+	..	..	..	..	1	1	..	..
1825	—	..	..	+	..	..	..	..	1	1	..	..
1826	—	—	..	+	..	..	..	..	1	2	+ 6	−12
1827	—	+	..	+	..	..	..	..	2	1	..	..
1828	+	+	..	+	..	..	..	..	3		..	..
1829	+	+	..	—	..	..	..	..	2	1	..	..
1830 Max.	+	+	..	—	..	..	..	..	2	1	..	..
1831	+	+	..	+	..	—	..	..	3	1	+12	− 4
1832	+	+	..	+	..	—	..	..	3	1	..	..
1833 Min.	—	—	..	—	..	+	..	..	1	3	..	..
1834	—	—	..	—	..	+	..	..	1	3	+ 5	− 7
1835	—	—	..	—	..	+	..	..	1	3	..	..
1836	—	—	..	—	..	—	..	..		4	..	..
1837 Max.	—	—	—	—	—	—	..	..		7	..	..
1838	—	—	+	—	—	—	—	..	1	6	..	..
1839	—	—	—	—	=	—	+	..	1	5	..	..
1840	+	—	—	—	+	—	+	..	3	4	+ 6	−29
1841	+	=	—	—	+	—	+	..	3	3	..	..
1842	+	+	=	—	+	—	+	..	4	2	..	..
1843 Min.	+	+	=	—	—	—	+	..	3	3	..	..
1844	—	—	=	—	—	—	—	..		6	..	..
1845	—	—	—	+	—	+	—	..	2	5	+12	−19
1846	=	—	—	+	—	+	+	..	3	3	..	..
1847	+	—	—	+	+	+	+	..	5	2	..	..
1848 Max.	—	—	+	+	+	—	—	..	4	3	..	..
1849	+	=	+	+	+	—	—	..	4	2	..	..
1850	+	+	+	+	+	—	—	—	5	3	..	..
1851	+	+	+	+	+	—	—	—	5	3	..	..
1852	+	+	+	—	+	—	=	+	5	2	+31	−18
1853	+	—	=	—	=	—	=	+	2	3	..	..
1854	+	—	—	—	—	—	—	+	2	6	..	..
1855 Min.	—	—	—	—	—	—	—	+	1	7	..	..
1856	—	=	—	—	—	—	—	—	0	7	..	..
1857	+	+	+	—	—	—	—	—	3	5	+ 8	−28
1858	+	+	+	+	—	—	=	—	4	3	..	..
1859	+	+	=	+	—	—	+	—	5	2	..	..
1860 Max.	+	+	—	+	—	+	—	+	5	3	..	..
1861	+	+	=	+	—	—	+	+	5	1	..	..
1862	+	+	+	..	—	..	..	+	4	2	..	..
1863	+	+	+	—	..	+	..	—	4	2	..	..
1864	+	+	+	—	..	—	..	—	4	2	+31	−15
1865	+	+	+	—	..	+	..	=	4	1	..	..
1866	+	+	+	+	..	+	..	+	6	0	..	..
1867 Min.	+	+	+	—	..	—	+	+	5	2	+15	− 3

groups can generally be formed of maxima and minima years of sun-spot area. When the period is short triennial groups can be taken. This is the method adopted by Mr. Meldrum and other distinguished meteorologists.

Proceeding in this manner maxima and minima sun-spot sets of years are formed from 1808 to 1862 inclusive, then the sum of the excess or deficiency of the rainfall in respect to its mean, is found for each set of years for the regions comprised in the I, II and IV territorial groups of Mr. Schott's tables.

The results obtained are shown in the following table: (Table II, page 98).

In this table we have five maxima sets of years and five minima, as respects the extent of the sun-spots. In the first territorial group, comprising the seacoast from Maine to Virginia, the results in the maxima years conform to the supposed law three times, and twice are unconformable. In the minima years they are four times conformable, and once unconformable.

In the second territorial group, embracing New York State and the adjacent parts of Canada, Massachusetts, Vermont, and New Hampshire, the results in the maxima years agree with the law twice and disagree twice, and in the minima years they accord with the law twice and are opposed to it once. The observations in this group extend over only four maxima and three minima sets of years.

In the fourth territorial group, comprising Ohio Valley, Ohio, Indiana, Illinois, Kentucky, and part of Missouri, the observations extend over four maxima and four minima sets of years. The results in the maxima sets conform to the law twice, and twice are unconformable. While in the minima set of years, they are in accordance with it three times and are unfavorable once.

If we now take the sum of the results, embracing all the three territorial groups, it is seen that in the five maxima sets of years there is an excess of rainfall three times, and a deficiency twice, and that in the five minima sets of years, there is a deficiency five times, the result being in entire conformity to the supposed law.

Yet amid these results striking anomalies are found, for in the maxima set of years extending from 1814 to 1818, there is a deficiency of 31 per cent. of the mean; and from 1833 to 1839, which are maxima years, the three territorial groups all present results below the mean, the first giving —·03, the second —·60 and the last —·55.

In view of the results obtained from these two modes of investigation I think we may venture to infer, that, so far as trustworthy observations have been made throughout the United States, they point to a connection existing between the variations in the sun-spot area and those of the annual rainfall; the rainfall tending to rise above the mean when the sun-spot area is in excess and to fall below when there is a deficiency of solar activity.

TABLE II. Sum of the excess or deficiency of the annual rainfalls of the groups as compared with the mean annual rainfall and expressed in the percentage of the latter.

Quinquennial and Triennial groups of maxima and minima years of sun spot area.	I Sea coast of Maine to Virginia.	II State of New York and adjacent parts of Canada, N. H., Mass., and Vt.	IV Ohio Valley Ohio, Ind., Ill., Kent., and part of Missouri.	Total.
1808 1809 1810 Minimum 1811 1812	−·02	..	..	−·02
1814 1815 1816 Maximum 1817 1818	−·31	..	..	−·31
1821 1822 1823 Minimum 1824 1825	−·30	..	+·22	−·08
1829 1830 Maximum 1831	+·27	+·05	−·07	+·25
1832 1833 Minimum 1834	−·03	−·11	−·08	−·22
1835 1836 1837 Maximum 1838 1839	−·03	−·60	−·55	−1·18
1841 1842 1843 Minimum 1844 1845	+·08	−·08	−·04	−·04
1846 1847 1848 Maximum 1849 1850	+·06	−·05	+·53	+·54
1855 1856 Minimum 1857	−·02	+·02	−·14	−·14
1858 1859 1860 Maximum 1861 1862	+·41	+·27	+·21	+·89

VELOCITY OF PRIMITIVE UNDULATIONS. By PLINY EARLE CHASE, of Haverford College, Pa.

NEWTON'S hypothesis of a primitive æthereal energy, as the source of cosmical attraction, underlies the fifty theses[1] which I have submitted as a contribution to the discussion of Anderssohn's "*Lösung des Problems über den Sitz und das Wesen der Anziehung*," at the coming Breslau meeting of the German Association of Naturalists and Physicians. In those theses I have embodied some of the most important conclusions to be drawn from the physical investigations in which I have been engaged during the past twelve years, and I desire to invite special attention to the curious analogy in the closing thesis, which may, perhaps, furnish an important clew to the unity of forces that so many are seeking, and which Secchi has made the subject of his latest work.

Henry's experiments have shown that there may be ponderable resistance occasioned by imponderable agency; Plateau's, that motion may shape cosmical masses; Anderssohn's, that so-called attractions may be explained by thrusts as well as by pulls; my own, that purely mechanical vibrations may produce polarity. The tendency of modern theorizing to resolve all physical agencies into forms of motion, suggests the inquiry whether the primitive motion, of which gravity and other forces may be special developments, has a constant, or a variable, velocity. The hypothesis of constancy seems to me more probable *a priori*, and the subordinate variability, directly as the mass and inversely as the square of radius, can be readily explained in accordance with that hypothesis; inasmuch as the number of radial impulses, intercepted by a given surface, varies as the inverse square of the distance, and the inertia varies as the mass. Galileo's law of rectilinear motion, Newton's laws of motions with reference to centres of force, and the resulting laws of curvilinear motion under combined tangential and focal impulses, are all simple corollaries from the hypothesis of a universally oscillating, perfectly elastic, imponderable, material æther, which would necessarily establish special systems of centripetal and centrifugal undulations about every centre of gross inertia. We may, therefore, reasonably seek for the value of the hypothetical constant velocity, and in order that our terms may all be strictly comparable, we will look

[1] Proc. Amer. Phil. Soc., xiv, 141-7.

merely at radial action, either towards the sun's centre, or towards the central sun.

Let $2F = M =$ modulus of Newtonian æther $=$ height of a homogeneous æthereal atmosphere at sun's surface, which would propagate undulations, or motion towards a vacuum, with the primitive constant velocity $= 2 \times$ virtual fall which would produce the same constant velocity.

" $2r =$ diameter of sun at any given time, either actual, or when homogeneously expanded or contracted to any point in space; $r_\prime = nr_\circ$ being the present radius, and $r_\circ$ the radial unit.

" $2r_\circ = v_\circ =$ diameter of sun at which the æthereal and gravitating motions would all be equal, and $2F$ would $= 2r = v_\circ = v_\prime = v_{\prime\prime} = v_{\prime\prime\prime}$; $v_\circ$ being the limit of possible æthereal velocity $=$ primitive constant velocity, expressed in the unit of time of traversing $2r_\circ$.

" $v_\prime = 2r\sqrt{\frac{f}{r}} = \sqrt{2gr} =$ velocity communicated by infinite fall to sun's surface (present, past or future; actual, expanded or contracted).

" $v_{\prime\prime} = 2\frac{v}{\pi_\rho} = \frac{4r}{t_\rho} =$ mean velocity of grand-central undulation accordant with v_ρ (v_ρ being superficial equatorial velocity, and t_ρ being time of solar rotation).

" $v_{\prime\prime\prime} = 2f = 2f_\circ t^2 = g = \sqrt{2gf} = 2r_a =$ falling velocity communicated, at sun's equatorial surface, by virtual fall through the radius of a circumference equivalent to a primitive wave-length $=$ focal distance of vertex of a corresponding parabolic wave. Then $g = \frac{v_\circ^2}{2F} = \frac{v_\prime^2}{2r} = \frac{nv_{\prime\prime}^2}{2r}$ $= \frac{v_{\prime\prime\prime}^2}{2f}$, and we have the following geometrical series:

M	$\propto$	r^2	$M_\prime = 2r_\circ n^2$	$=$	473800	$r_\prime$	
—	$\propto$	$r^{\frac{3}{2}}$	$— = 2r_\circ n^{\frac{3}{2}}$	$=$	973.3	$r_\prime$	
$2r$	$\propto$	r	$2r_\prime = 2r_\circ n$	$=$	2	$r_\prime$	
—	$\propto$	$r^{\frac{1}{2}}$	$— = 2r_\circ n^{\frac{1}{2}}$	$=$	$r_\prime$	$\div$	243.3
$v_\circ$	$\propto$	r°	$v_\circ = 2r_\circ n^\circ$	$=$	$r_\prime$	$\div$	118450
$v_\prime$	$\propto$	$r^{-\frac{1}{2}}$	$v_\prime = 2r_\circ n^{-\frac{1}{2}}$	$=$	$r_\prime$	$\div$	57640000
$v_{\prime\prime}$	$\propto$	r^{-1}	$v_{\prime\prime} = 2r_\circ n^{-1}$	$=$	$r_\prime$	$\div$	28055000000
—	$\propto$	$r^{-\frac{3}{2}}$	$— = 2r_\circ n^{-\frac{3}{2}}$	$=$	$r_\prime$	$\div$	13654000000000
$v_{\prime\prime\prime}$	$\propto$	r^{-2}	$v_{\prime\prime\prime} = 2r_\circ n^{-2}$	$=$	$r_\prime$	$\div$	6645600000000000

The values in the right hand column are determined by the constant ratio $n^{\frac{1}{2}} = 486.7$, which is approximately found by dividing the known value of $v_{/}$ by the approximately known value of $v_{//}$, as follows.

The approximate values of t_{ρ} vary between the estimates of Spörer (2127554 sec.), and that of Herschel, Bianchi and Laugier (2188080 sec.). The respective corresponding values of $v_{//}$ are $\frac{r_{/}}{531888}$ and $\frac{r_{/}}{547020}$ in one second of time. Earth's mean distance from sun being $214.86r_{/}$, we find $v_{/} = \frac{2\pi r_{/} \sqrt{2}}{1\text{yr} \div 214.86^{\frac{3}{2}}} = .000887r_{/}$ per second; $g = \frac{4\pi^2 r_{/}}{(1\text{yr} \div 214.86^{\frac{3}{2}})^2} = .000000393r_{/}$ per second; $n^{\frac{1}{2}} = v_{/} \div v_{//} = 471.7$ according to Spörer, 478.3 according to Carrington, 480.3 according to Faye, 485.1 according to Herschel, or, as will be shown below, 486.7, if the velocity of primitive undulation is identical with the velocity of light. Then $v_{\circ} = v_{/} n^{\frac{1}{2}} = .4183r_{/}$ per second (S.), $.4242r_{/}$ (C.), $.4295r_{/}$ (F.), $.4302r_{/}$ (H.), or $.4316r_{/}$ (v. l.).

The value $v_{///} = 2r_{a}$ represents the following comparative considerations of rectilinear and curvilinear motion. Let

$v_{\circ}$ = linear velocity communicated by virtual fall through $\frac{M}{2}$ = circular velocity acquired by virtual fall through $\frac{r_{\circ}}{2}$, or through (circumference $\div 4\pi$), in the time of traversing $r_{\circ}$ by revolution in a circular orbit = parabolic perihelion velocity, or velocity of infinite fall, at a distance of $2r_{\circ}$ from the attracting centre.

w = a circular wave of same length as the primitive wave, = wave generated by a single centripetal impulse.

$\frac{g}{2} = \frac{w}{4\pi}$ = primary virtual fall corresponding to a single wave-length.

Then $w = 4\pi r_{/} \div n^3 = r_{/} \div 1057700000000000$, if $n^{\frac{1}{2}} = 486.7$. If we adopt other possible values of $n^{\frac{1}{2}}$, the number of wave-lengths per second may be provisionally estimated as between $.4183r_{/} \div w$ (S.) and $.4302r_{/} \div w$ (H.), or between $367\ (10)^{12}$ and $446\ (10)^{12}$. These extreme estimates both correspond, in rapidity, to undulations near the hottest portion of the solar spectrum,[2] and indicate *a possible complete identification of the sun's attractive and thermo-dynamic energies.*

[2] If the identification is exact, the actual number of oscillations per second is $457\ (10)^{12}$; the number in the extreme end ray being $440\ (10)^{12}$ if sun's mean distance is 92,000,000 miles.

According to Struve's constant of aberration, v_λ (the velocity of light) $= r \div \frac{497.83}{214.86} = .4316r$ per second. The corresponding value of $n^2 = 486.7$, and the number of vibrations per second $=$ $4.57\ (10)^{12}$, as before stated. Therefore the constant velocity of primitive æthereal impulsion, v_o, which would account for all the gravitating motions of the solar system, *is almost, if not exactly, identical with the velocity of light;* the several approximate velocities being, in units of light-velocity,

According to Spörer	.969
" " Carrington	.983
" " Faye	.987
" " Herschel	.997
Light	1.000

These values exhibit a discrepancy, varying between $\frac{1}{4}$ of one per cent. (H.), and 3.1 per cent. (S.). If this close accordance is significant of actual physical influence, there are various uncertainties of observation, in the elements of the calculation, which might account for a still greater discrepancy. Perhaps the most important is the uncertainty as to the proper allowance for the differences in the angular velocity of sunspots in different latitudes. If there were no resistance to the revolution of the spots their velocity should be planetary, and therefore far greater than it actually is. On the other hand, if they were rigidly embedded in a rotating mass, their angular velocity should be uniform. The differences are such as to suggest the possibility that the velocity may be partly planetary and partly rotary, and in that case it is not unlikely that even Herschel's estimate of v_o may be one-third of one per cent. too great. The well-known thermo-dynamic principles which point to a gaseous structure of the sun, as well as Prof. Young's observations on the force of solar explosions, seem to indicate so nice an adjustment of centripetal and centrifugal forces within the sun's mass, as to strengthen the hypothesis that its mean mass, its mass-fluctuations, the varying motions of its spots, and the varying orbital eccentricities of its attendant planets and satellites, may all be continually determined by the constant primitive motion.

The three following additional relationships may, perhaps, prove to be something more than merely curious.

1. The common ratio of the geometrical series, $n^{\frac{1}{4}}$, is very nearly, if not exactly, $\sqrt{.4\,\frac{\text{mass of sun}}{\text{mass of planets}}}$; $r\sqrt{.4}$ being the radius of spherical gyration.

2. The number of vibrations of the mean caloriferous rays in the unit of time which satisfies all the equations of the geometrical series, is nearly, if not exactly, the cube of $\frac{2 \times \text{sun's mass}}{\text{Jupiter's mass}}$.

3. If the sun's velocity of equatorial rotation be regarded as the velocity at the vertex of a parabolic wave, and consequently, as measured by the focal distance of the vertex,[3] the motion of the sun's centre in space will be measured by the parameter of the same parabola. This is in weighty accordance with the stellar and planetary correlations which I communicated to the American Philosophical Society[4] on the 20th Sept., 1872.

If we were to suppose a slight æthereal expansion by the intense heat at the sun's surface, the inrushing colder æther might produce gyrations analogous to our atmospheric cyclones. But independent of any such hypothesis, it is by no means certain that there may not be some slight progressive æthereal motion, combined with the motion of wave-form, or some radial oscillation accompanying the transverse light-undulations. Any such progressive motion, capable of communicating, at each wave-impulse, a velocity $= \frac{v_A}{685\,(10)^{18}}$, would suffice to produce the greatest manifestation of gravitating force in the solar system. In pursuing any investigations for the further elucidation of the subject, due regard should be paid to the double system of centripetal and centrifugal waves originated by inert matter-molecules, and to the æthereal properties which admit of Laplace's supposed instantaneous transmission of gravitating action.[5] It is well also to consider that all the permanent gravitating motions, represented by $v_{,}$, $v_{,,}$, tend to perpetual oscillation, and may therefore be subjected to all the laws of oscillation.

Whatever difficulties we may find in trying fully to account for the foregoing accordances, those accordances are well-established FACTS in nature. They are facts in perfect keeping with Newton's

[3] $g = 2f$; $\therefore \sqrt{2\,g\,h} = 2\sqrt{f\,h}$.

[4] Proc. A. P. S., xii, 518-22.

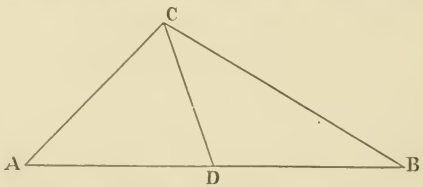
[5] But not necessarily of gravitating *motion*.

original hypothesis, as well as with the hydro-dynamic and mechanical investigations of Challis, Norton and Leray, with the experiments upon cosmo-plastic forces, and with the various harmonies of cosmical and molecular mass and motion which I have published elsewhere; facts which seem to me to lend new probability, if not moral certainty, to the belief in a discoverable and measurable uniform origin of all physical forces.

A NEW DEMONSTRATION OF AN OLD THEOREM IN GEOMETRY. By BENJ. S. HEDRICK, of Washington, D. C.

"THE sum of the three angles of any triangle is equal to two right angles."

In any triangle, A B C, from either of the angles as C, a line C D, may be drawn to the opposite side. This will form two triangles having for angles the sum of the angles of the original tri-



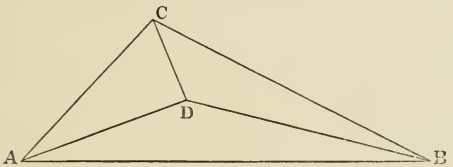
angle, and in addition two new angles A D C + C D B = two right angles. But as the original triangle may be any triangle whatever, and as the line C D may be drawn to any point of the opposite side, every possible form of triangle may be formed, and therefore one exactly similar to the original triangle. Either of these new triangles may be treated in the same way, that is, a line may be drawn from either of the angles to the opposite side so as to make three, four, or more triangles up to infinity. Each time a line is drawn in this way two new angles are added, the sum of which is equal to two right angles, and another triangle is added to the number. As then the addition of an amount of angle equal

to two right angles forms every possible form of triangle, the sum of the angles of any triangle must be equal to two right angles.

When the original triangle is equilateral a triangle similar to the original could be formed only when three or more triangles are formed.

The demonstration may be modified as follows:

Take D at any point within the triangle and draw lines to each of the angles. Three triangles are thus formed having the sum of the original angles together with three angles A D B, B D C and C D A equal to four right angles. As the original triangle may



be any whatever, and D taken at any point whatever within the triangle, every possible form of triangle may be formed, and therefore one similar to the original. But there are three triangles now and an amount of angle equal to the angles of the original triangle plus four right angles.

The sum of the angles of the original triangle must therefore be equal to two right angles.

This method makes the demonstration as it were, axiomatic. The truth of the proposition is grasped by a single thought. It is important in mathematical training to teach the student to take long steps, to see through a demonstration from end to end. If steps are multiplied the mind loses itself in a labyrinth of propositions. Hence a great deal of the confusion that leads men to believe in false conclusions. But by reducing all the steps to clear intuitions, the whole way is clear from the beginning to the end. The conclusion is perceived as an axiom. The method of Euclid, whilst it serves to teach the uninformed to reason mathematically, does not give the grasp and penetration necessary in pursuing the sort of reasoning required in matters of science and business; and hence the objection of abstract metaphysicians like Sir Wm. Hamilton to mathematics as a training.

On the Cement of some Natural and Artificial Stones. By T. Sterry Hunt, of Boston, Mass.

ABSTRACT.

The speaker noticed the cementing material of various sandstones and conglomerates, which is sometimes carbonate of lime, sometimes silica, and at other times, as shown by A. A. Hayes, a silicate of magnesia. He then referred to a series of experiments published by him in the Proceedings of the Royal Society fifteen years since, in which he had described the reaction between silica, in the form of quartz, and the carbonates of lime and magnesia in the presence of heated solutions of carbonate of soda. The latter has the power to dissolve quartz, though slowly, forming a soluble silicate, which, reacting upon the earthy carbonates, gives rise to insoluble silicates of the earthy bases, with regeneration of the alkaline carbonate; which thus acts as a medium to bring about the union of these insoluble substances. The speaker had at that time, and in subsequent publications, pointed out that this reaction plays an important part in the local alteration of uncrystalline sedimentary strata in proximity to eruptive rocks or to thermal springs, generating in the sediments many crystalline silicates.

Another series of studies on the artificial formation of mineral silicates has been made with a different object by Mr. Frederick Ransome, who has sought to employ these silicates for cementing together sand to form artificial stone, and has obtained very curious and important results. The reaction between solution of silicate of soda and chloride of calcium gives rise to an insoluble precipitate of silicate of lime, but it is a remarkable fact that while this precipitate formed in the midst of a liquid, exhibits no cohesion, it becomes, when generated by this double decomposition in the pores of a mass of sand, a strong cementing material, binding together the particles of the latter so as to form an artificial stone. The chloride of sodium, which is at the same time formed, is subsequently washed out from the pores of the mass, and the material thus prepared is now largely used for building purposes as an artificial stone.

In my experiments already referred to, a very sparingly soluble form of silica was generally taken (though artificially prepared silica was also employed), and the alkali, like the earthy base,

was in the state of carbonate. If for these could be substituted a soluble silica, and bases in an uncombined state, a much more rapid reaction would be insured. Mr. Ransome has attained these conditions by using a mixture of such silica and quicklime with a portion of dissolved silicate of soda. This latter reacts with the lime to give a silicate of lime, with liberation of caustic soda, which attacks the soluble silica to form a fresh portion of silicate of soda. While, in my experiments, the soda was liberated as a soluble carbonate, it is found that in the absence of carbonate of lime a portion of the soda combines with the lime and silica to form a hydrated double silicate. From this it results that if the ingredients are in proper proportions, the whole of the soda and lime are at last fixed in the form of this insoluble double silicate, related to pectolite in composition, which makes, like the simple silicate of lime, a strong cement. By this means Mr. Ransome has succeeded in preparing a new kind of artificial stone, which has received the name of apoenite. It contains no soluble matter to be washed out, and promises to be much superior to the stone prepared with the aid of chloride of calcium.

The relations of the reactions above described to the cementing together of the components of rocks in nature are not difficult to understand when we consider that most natural waters hold in solution salts of lime, and that alkaline silicates are liberated by the decomposition of feldspathic minerals.

The Sewage Question Chemically Considered. By T. Sterry Hunt, of Boston, Mass.

ABSTRACT.

Few questions are more important than this for our modern civilization from a sanitary point of view. Excrementitious matters poison the soil and the air of our towns, and contaminate our sources of water-supply. In a densely populated country like England, the importance of getting rid of these substances has been

more urgently felt than among us, but already in our great centres it is forcing itself upon our attention, while many of our watering-places and sea-side resorts, often with very inadequate drainage, are in danger of becoming seats of pestilence. The system of getting rid of sewage by water has at first sight much to recommend it, but the pollution by this means of the waters near great towns is a very serious evil. The plan sometimes adopted in England of using sewage-waters for irrigation presents many objections besides its great cost. The contamination by it, of the air and of the green crops is a serious evil while all the methods of purifying, by chemical means, the sewage-waters before turning them into the rivers are confessedly failures. The earth-closet system has much to recommend it, but requires the transport of an amount of earth to and from the towns which would make its extended use extremely costly and difficult of execution, while, unless great care is taken to secure the proper kind of earth, its disinfecting action is imperfect. In an economic point of view, it has the great advantage of returning directly to the soil the fertilizing elements, the annual value of which in England has been estimated at eight shillings sterling for each person, a figure probably considerably below its present commercial value.

Besides the methods by water and by earth, there is a third one —by fire. Attention has often been called of late to the efficiency of this purifying agent in connection with cremation, but a more important and a greater source of danger than our cemeteries is everywhere around us, so that the possibility of applying fire to the destruction of excrementitious matters should not be lost sight of. Such a result has already been attained in England by a modification of the earth-closet system, in which for dry earth is substituted finely-divided charcoal, a substance well known as one of the most efficient antiseptics and deodorizers. For this purpose the charcoal obtained by the charring of sea-weed, in a process for the extraction of iodine, was first used, but can be advantageously replaced by that furnished by street-sweepings. From its greater lightness and superior absorbent power, only one-fourth as much of this charcoal as of earth is required, and the odorless and partially-dried mixture is removed from time to time, and charred by heating it to redness in close vessels like the retorts used in making coal-gas. By this means

all organic and volatile matters are destroyed and expelled in the forms of water, ammonia, acetic acid, tar and gas; and the charcoal, augmented in quantity, is immediately ready for use again. It, however, retains all the alkalies, earths and phosphates, which give it a great fertilizing power; so that after a time it is found economical to use it as a manure, and to substitute freshly prepared charcoal in its place. From the products of this decomposition by heat acetate of lime is manufactured, while the whole of the ammonia of the sewage is converted into sulphate of ammonia, a salt of great fertilizing power and of considerable commercial value. In brief, this process renders the sewage inodorous until it is destroyed by fire and converted into products not only harmless, but precious for agriculture.

This system, devised for Mr. Stanford, an English chemist, has now been tried on a large scale with very satisfactory results, it is claimed, for three or four years at Dalmuir, near Glasgow, and we are informed has lately been adopted by the authorities of one or more large towns in England. While its efficiency is undoubted it is declared that its execution presents few practical difficulties, and the commercial value of the products is said to be such as to make the process a source of pecuniary profit.

Pure Carbonate of Soda. By J. Lawrence Smith, of Louisville, Ky.

ABSTRACT.

For many years all the carbonate of soda used by me in mineral analysis has been prepared in the following method, viz., by making oxalate of soda and then decomposing it by heat. It can be described in the shortest possible manner by giving the figures and method employed for obtaining a given result. The carbonate of soda commonly used has been the crystals of ordinary sal soda, washed with a little water to detach the adhering dust, or if one has pure soda at his command it can be used to advantage. The oxalic acid used is the ordinary oxalic acid of

the shops once recrystallized, of which recrystallized acid I always have a supply of several pounds in my laboratory.

Sixty-three grammes of oxalic acid and 143 grammes of sal soda are dissolved by heat in 200 c. c. m. of distilled water. Filter the solutions if necessary. To the solution of soda, when cold, add the solution of oxalic acid, just hot enough to keep from crystallizing; add it by degrees, stirring well; after the mixture is completed, it is expected that the solution will have an alkaline reaction, to keep any trace of soda in solution; the oxalate of soda will be precipitated in great part shortly after the operation is completed; let stand for a short while to cool completely, decant the supernatant liquid, add a little distilled water, break up with a stirrer the lumps of crystals that may have formed, throw on a filter over a Bunsen aspirator, using a six-inch filter, wash with about a half litre of distilled water, and let dry. This may be placed aside in a glass bottle if not needed at once for forming carbonate of soda; the quantity of dry oxalate produced is thirty grammes. To convert into carbonate project the oxalate little by little into a platinum capsule over a good-sized Bunsen burner; after being strongly heated, the oxalate is decomposed into the carbonate, and, if heated high enough to be fused, will furnish about twenty-three grammes of fused carbonate of soda; fused or not, dissolve in water, filter, evaporate to dryness, de-hydrate over a naked flame, and granulate it by stirring when hot.

Double or quadruple the quantities above given may be operated upon at once with similar results. The carbonate of soda thus made is perfectly free from chlorine, sulphuric acid, silica, or other impurity that will interfere with its use in analysis.

PURE CARBONATE OF POTASH. By J. LAWRENCE SMITH, of Louisville, Ky.

ABSTRACT.

IT may be wrong to use the word pure in connection with the preparation of this substance in the manner to be described, as it may contain at the end of the operation a trace of nitrate of potash. The starting point is pure nitre, which is a cheap potash

salt, and can be readily purified by repeated crystallization; the other is oxalic acid, the commercial acid recrystallized once or twice; fifty grammes of pure nitre and 100 grammes oxalic acid are placed in a platinum capsule; to this is added a small quantity of water, and heated over a gas burner; before the mixture is entirely dry, a second portion of water is added and the heat continued until the mass is brought to dryness, at which time nearly all the nitric acid of the nitre is expelled; the heat is now continued, and the whole mass brought to redness, breaking up the lumps with an iron rod, when the oxalate of potash formed will be decomposed into the carbonate; the mass is treated with water, filtered, dried, and granulated over the flame; this furnishes about thirty-one grammes of carbonate of potash which, as I have already said, may contain a little nitre, but this in no way interferes with the ordinary use of carbonate of potash in making fusions. For this purpose I commonly mix equal parts of carbonates of soda and potash at the time they are required for use.

Absolute Alcohol. By J. Lawrence Smith, of Louisville, Ky.

ABSTRACT.

This substance, as obtained in commerce, very seldom marks more than ninety-eight to ninety-nine per cent. It is, however, not unfrequently made in our laboratories, and when this is done the usual method is employed of pouring strong alcohol on lime until the lumps of lime are covered. This method of proceeding gives a thick magma which, when heated over a water-bath, allows the alcohol to pass over but slowly, and much of the alcohol is lost from the impossibility of the heat penetrating the thick mass. The method I follow differs from this in no way except in the quantity of lime employed; using the smallest quantity of lime necessary to abstract all the water, it is surprising how complete the lime will perform its function in this respect. Take, for instance, one litre of alcohol of ninety-four per cent.; this contains about sixty grammes of water; if to this be added 120 grammes

of good and fresh burnt lime, requiring about forty grammes of water to convert it into hydrate, actual experiment proves that when kept in contact with the alcohol a sufficient length of time it accomplishes this absorption of water, and the alcohol decanted from the precipitated lime will be fully ninety-eight per cent.

Operating upon this fact, I have been long in the habit of supplying myself with alcohol of ninety-eight and 100 per cent., by proceeding in the following manner: I have in my laboratory three or four two-litre bottles, into each of which I place 1½ litre of ninety-four per cent. alcohol, the strongest alcohol sold in commerce; to this is added 180 grammes of fresh burnt lime of the best quality broken up into a coarse powder. These bottles are set aside on the shelf and agitated from time to time; the oftener this is done the more rapid will the reaction be accomplished. A week or ten days will usually suffice, when the bottles are allowed to remain at rest and the hydrate of lime will settle in a few days, and by a siphon two-thirds of the original alcohol can be drawn off free from lime, which marks ninety-eight per cent. alcohol, and when filtered, and fifty c. c. m. evaporated to dryness there will be left only the merest trace of lime, less than one-half milligramme. But, of course, redistillation is so simple that if we wish the alcohol at 98° it can be readily distilled over a water-bath. The magma remaining in the bottle, when distilled over a water-bath, furnishes the remainder of the alcohol about one-half per cent. higher.

When absolute alcohol is desired, take the alcohol just as it has been siphoned off or distilled from the magma, put it in a convenient flask for distillation, and to each litre add 120 grammes of lime in coarse powder, attach to a Liebig condenser inverted, so that the alcohol will run back into the flask when condensed: this is continued for an hour and a half or two hours. The condenser is then placed in its normal condition and alcohol distilled over which will mark 100 per cent. Recently I have learned that there is a method adopted of making the absolute alcohol by one distillation, operating by the inverted condenser first, but in this process the amount of lime called for is the usual quantity, whereas I find that by reducing the lime to its minimum and always having bottles ready to furnish ninety-eight per cent. alcohol, the operation is facilitated, and the loss diminished. So that with the ordinary conveniences and appliances of the labora-

tory, that are always at hand to be mounted, I can, with fifteen or twenty minutes of *personal attention and manipulation*, obtain a litre or two of absolute alcohol. Of course the time for the reaction of the materials and the distillation is not referred to, as this requires little or no supervision.

On a New Way of Illustrating the Vibrations of the Air in Organ Pipes. By Joseph Lovering, of Cambridge, Mass.

I shall begin with a brief description of the methods adopted by W. B. Rogers and Koenig for making visible the vibrations of the invisible air in organ pipes. By the first method two gas burners were fed from a common reservoir; a glass tube was placed over one burner and the length of the flame altered until it was brought into unison with the fundamental note of the tube, and an energetic vibration of the flame and the column of air began. The flame on the other burner also vibrated from sympathy. The individual vibrations of this naked flame were unravelled by making the burner revolve in a small circle of about one inch in diameter. A cylindrical sheet of light resulted from the motion which was channelled by dark spaces as soon as the sound was heard. The method has the advantage of giving an object which can be seen in all directions, with the disadvantage, however, on account of the small circle in which the flame travels, of crowding upon each other the alternate dark and bright spaces of the fluted surface.

The description just given has reference to the apparatus constructed by Koenig under the name of the *Appareil à flammes chantantes de M. le comte Schaffgotsch*. But I have not been able to find in Schaffgotsch's account of his apparatus and experiments any allusion to this method of exhibiting to the eye the individual vibrations by the revolution of the flame itself. Probably, it was an appendage to Schaffgotsch's apparatus suggested to Koenig by the experiment of Prof. W. B. Rogers.[1]

[1] Amer. Journ. Science, xxvi, 1858.

In this experiment only a single flame was employed; a tube being placed over it large enough to allow the interior flame to revolve in a small orbit.

The second method for accomplishing the same object is a device by Koenig, and is known under the name of the *Manometric Flames*.[2] An opening is made in the side of an organ pipe at the place where the greatest changes of density occur when the pipe is sounding: to this hole is applied a small box, the end which is placed upon the hole being covered with a delicate membrane. Gas is introduced into this box by one tube and then conducted from the box to the burner by another tube. The vibrating air in the pipe plays upon the membrane, and communicates vibration to the flame. The individual vibrations of the flame are revealed to the eye, by reflecting it in a revolving mirror after the manner of Wheatstone. Four mirrors placed upon the four vertical sides of a revolving cube produce the same effect with a smaller velocity. This method has the disadvantage of substituting for the flame itself a faint and virtual image, not easily seen in daylight or in all directions. It has the advantage of admitting of a large orbit of revolution and thus separating widely the individual vibrations of even high notes.

I have endeavored to combine the advantages of both methods without incurring their disadvantages. Imagine eight arms of gas pipe, each one foot in length, arranged as the spokes of a horizontal wheel upon a hollow hub, which is lengthened out below to the extent of six inches. This hub is closed at the upper end by steel and is balanced upon a steel point at the top of a hollow upright through which the gas is delivered. Two holes are made in the sides near the top, through which the gas flows out into the hollow hub and the attached arms. The upright comes up through the bottom of a vessel, into which water is poured in sufficient quantity to immerse the bottom of the hub and prevent leakage of the gas below. In this way great freedom of motion is secured on the steel point, with a perfectly tight joint. The arms are rotated in a large circle by clock-work, and their weight suffices to make them serve as a fly-wheel and acquire a uniform motion. The arms are closed at the ends, and the gas issues in eight vertical streams from as many holes in the tops of the arms. When these

[2] Ann. Chem. und Phys. cxxii.

streams are lighted, the rotation produces a large cylindrical sheet of light. The greater the number of arms the smaller is the velocity required to produce persistency of impression in the eye, with the additional benefit of having a well balanced instrument. Vibration of the flames is produced by attachments to the organ pipes similar to those used by Koenig. When the full pressure of the gas is on, the cylindrical sheet of light is three or four inches in height, and, when the organ pipe speaks, it is broken up as neatly as when the virtual image is used in Koenig's apparatus. The effect can be seen readily in full daylight and from every part of a large room. The best effect is with organ pipes at least three feet in length and open at both ends. Of course, the serration is finer when shorter pipes are used. The experiment is very beautiful, even with short pipes, if the flame is brought down to the size of a pea. In this case the illuminated circle, as soon as the sound begins, is broken up into bright beads loosely strung upon a dark cord. On one side of each flame a screen is placed, which moves on a hinge and can be set at any angle with the plane of rotation. These screens serve the double purpose of checking the velocity, if the energy of the clock movement is too great, and of preventing the flame from flaring if the motion is too rapid. For purposes of research, Koenig's apparatus may be all that is desired. But for illustration in class rooms, and especially to large numbers, the new arrangement just described will be found to possess great advantages.

On the Number and Distribution of the Bright Fixed Stars.

By B. A. Gould, of Boston, Mass.

The magnificent work of Argelander entitled "Durchmusterung des nordlichen Himmels," is well known to lovers of astronomy. His problem was no less than the formation of a complete list of all the stars of the northern hemisphere to the 9th magnitude inclusive, together with as many as possible of the 9–10 magnitude. The undertaking was successfully carried out, affording not

only an exhaustive series of charts, but likewise a "working-list," which an association of northern observatories is now employing for the determination of the accurate positions of the 315,000 stars which it contains. It furthermore records the aspect of the visible heavens at the time, with an accuracy amply sufficient for all purposes which do not require minute precision.

In this work, the magnitude of each star was estimated to the nearest half unit as it passed through the field of view; and since all the stars were observed more than once, and most of them several times, the mean of the several estimates was taken and is given in the published catalogue to the nearest tenth of a unit. In 1869 Professor Littrow of Vienna made a careful enumeration of the number thus given for each degree of magnitude, in order to ascertain how far the results would indicate an approximate uniformity of distribution for the stars lying within the portion of space under consideration.

Could we assume, 1st, that, in general, the apparent brightness of a star depends upon its distance from us, all being of the same order of intrinsic brilliancy, and 2dly, that stars are distributed through space with approximate uniformity, the total number visible, down to any given magnitude, would be proportional to the contents of the sphere within which stars of this magnitude are contained. Thus the degree of approximation to the truth, which is fairly attributable to the hypothesis mentioned, may be inferred from the degree of accordance which can be obtained between two series of numbers, the one representing the number of stars of each successive order of magnitude actually existing in the heavens, and the other being proportional to the contents of those spherical shells whose limiting radii correspond to these respective magnitudes. If any accordance at all satisfactory should be found to exist, it would render the assumptions probable to a corresponding extent, and would afford at the same time an independent, even though indirect and crude, determination of the constant ratio between the amounts of light which belong to two successive magnitudes, and thereby an indication of the relative distances of the stars.

If the data on which the inquiry is based be sufficiently extensive, we may reasonably expect that the effects of considerable deviations from perfect exactness in the general hypotheses will be masked in the totals, provided the assumptions be not essen-

tially erroneous. But there are serious difficulties in the way of an accurate comparison, arising from the inadequacy of our data, in consequence of inevitable errors in the estimates of magnitude, the moral impossibility of maintaining exactly the same ratio for different degrees of brightness, and other almost equally important obstacles. Notwithstanding all these, Professor Littrow considers his results as justifying the belief that there is a considerable degree of general uniformity, both in the distribution and the intrinsic brilliancy of the fixed stars which belong within the limits of this investigation; sufficient, indeed, to warrant an application of the formulas, resulting from this inquiry, to regions outside of the distance of 9th magnitude stars. From a discussion of the numbers given by the Durchmusterung as far as the 8th magnitude, assorted according to whole units, he obtains the value 0·423 as the ratio of brilliancy between typical stars of two successive magnitudes; and similarly from an assortment by half magnitudes, comprising all stars to the 8th magnitude inclusive, he obtains the value 0·431. Each of these computations gives, for the average distance of a star of the 9th magnitude, about 26 times that of the most distant 1st magnitude star.

The discordances between the numbers obtained by enumeration from the Durchmusterung and the empirical values deduced from Professor Littrow's formula, vary satisfactorily in sign; and the sums of the positive and of the negative residuals are nearly equal for the groups from $3\frac{1}{2}$ to $8\frac{1}{2}$ inclusive, when the residuals are reduced to fractions of the computed values. Still some of the discordances are large, the difference for stars of the 4th magnitude amounting to more than one-third part of the empirical number, while for those of the 9th magnitude it would exceed one-half of the whole. The appended table exhibits the number of stars for each half-unit of magnitude as obtained from the counting, and from Littrow's formula, according to which the number comprised within the half-magnitude m is given by the expression

$$Z_m = 0{\cdot}6098\,(3{\cdot}5295)^m.$$

To these I add the values corresponding to another formula, which I have deduced for the purpose of giving a somewhat better representation to the stars brighter than the $3\frac{1}{2}$ magnitude. In this the light-ratio for each magnitude is 0·4523 and the number of stars comprised in the half-magnitude m is

$$Z_m = 1{\cdot}0691\,(3{\cdot}2878)^m.$$

The residuals appended to each of the empirical series represent the excess of the computed numbers over the observed ones, expressed in hundredths of the former.

STARS OF THE NORTHERN HEMISPHERE.
According to the Durchmusterung.

Mag.	Counted.	Littrow's formula.	Difference.	New formula.	Difference.
1	6	3		5½	−11
1½	4	4		6½	+38
2	22	8		12	−90
2½	12	14	+14	21	+13
3	51	27	−89	38	−34
3½	60	50	−20	69	+13
4	128	95	−35	125	+ 2
4½	140	178	+21	232	+40
5	379	334	−13	411	+ 8
5½	463	628	+26	745	+37
6	1242	1179	− 5	1350	− 8
6½	2231	2215	− 1	2449	+ 9
7	4608	4161	−11	4441	− 3
7½	6878	7817	+12	8052	+14
8	14525	14686	+ 1	14600	+ 1
8½	28486	27590	− 2	26474	− 8
9	78185	51830		48003	

The marked defect of the computed numbers for stars of the 9th magnitude has led me to include the several values for this magnitude in the table, although they were intentionally omitted in deducing the formulas. I do not think that any expressions of the form ab^m can be found which will represent all the values to the 8½ magnitude inclusive, with an essentially better accordance. The numerical values of the residuals might be very slightly improved by using the method of least squares, but not to any important extent. The discordance of the formulas given would be still greater for stars of the 9½ magnitude, since Littrow's gives 97,370 stars and my own only 87,040, while in the Durchmusterung there are 177,505 stars of the 9·3, 9·4 and 9·5 magnitudes; those of the 9·6 and 9·7, which are needed in addition to make up the half-magnitude group, not having been observed.

It thus appears that, unless the fainter stars of the Durchmusterung have been estimated much too bright, the hypotheses in question cannot be extended beyond the $8\frac{1}{2}$ magnitude, even if they are to be regarded as approximately representing the tenth for the stars up to this limit, the distance corresponding to which would be, according to Littrow's formula, about twenty-two times, and according to the other formula about eighteen times, that of a star of the 1st magnitude.

The Uranometria Nova of Argelander, which gives the carefully observed magnitudes of all the stars which he could distinguish with the naked eye, afforded the standard for the magnitudes of the Durchmusterung; yet the first glance makes manifest large discrepancies between the rapidly-made estimates in the latter work and the sharp determinations of the former. The recently published revision and extension of the Uranometria, by Professor Heis of Aachen, assigns the magnitude to the nearest third of a unit for every star which he could discern, the lowest being $6\frac{1}{3}$, or one-third of a magnitude fainter than Argelander's inferior limit. The far greater precision of these determinations would give a more trustworthy basis for our inquiries than the Durchmusterung affords, were the numbers large enough to eliminate such irregularities as may justly be treated as accidental; but this seems not to be the case.

The completion of our Argentine Uranometry now augments the number of accurately determined stars, and renders it possible to assign the actual magnitudes for all stars throughout the heavens which are easily visible to the naked eye. I have taken much pains to secure an accordance between the adopted scale of magnitudes and that employed by Argelander in his Uranometry, and regard it as unlikely that the probable error of our individual magnitudes exceeds one-tenth of a unit. So too does Heis seem to have omitted no efforts for securing an accordance of his work with the same standard, and it is improbable that any essential error can exist in these estimates, made as they are by an astronomer of exceptional keenness of sight.

It is thus easy to ascertain the total number of stars in the firmament for each grade of magnitude within Heis's limits. The results are especially trustworthy, since every individual magnitude has been determined by careful and repeated comparisons with established standards and the same scale; and there now arises the interesting question, how far the accurate numbers given by

the two Uranometries, for stars as bright as the 6th magnitude throughout the entire heavens, would agree with the rougher estimates of a number of stars eleven times greater, but situated in the northern hemisphere only and including the $8\frac{1}{2}$ magnitude?

I have therefore devoted some labor to the independent determination of a formula which should represent as well as possible the results derived from the Uranometries alone, upon the hypothesis already stated. The best value which I have been able to deduce for the constant ratio of the light of stars of successive magnitudes is 0·4988, the degree of accordance of which with observation upon our fundamental assumptions may be inferred from the appended table, which gives the number of stars for each half unit of magnitude, as deduced from the Uranometries, and from the formula

$$Z_m = 3{\cdot}2384\ (3{\cdot}0184)^m,$$

together with the residuals, determined as in the preceding table. Those stars of Professor Heis's Uranometry which are given for the fractional thirds of a magnitude, are combined to form the numbers for the fractional halves in our table; a crude procedure, but the only practical one under the circumstances.

NUMBER OF STARS IN THE HEAVENS.

Mag.	Uranometries.			Formula.	Difference.
	N.	S.	Total		
1	8	6	14	$15\frac{1}{2}$	+10
$1\frac{1}{2}$	7	4	11	17	+35
2	25	20	45	$29\frac{1}{2}$	−53
$2\frac{1}{2}$	35	33	68	51	−33
3	55	41	96	89	− 8
$3\frac{1}{2}$	103	87	190	155	−23
4	132	108	240	269	+18
$4\frac{1}{2}$	254	154	408	467	+17
5	392	240	632	811	+22
$5\frac{1}{2}$	686	563	1250	1409	+16
6	1374	1075	2449	2449	0
$6\frac{1}{2}$	....	2022	....	4255	....
7	....	3317	....	7392	....

Various inferences are suggested by this table. The formula corresponding to our hypotheses differs greatly from the former ones, both in the coefficient and the ratio. The degree of accordance is not such as to warrant any great faith in the correctness of our assumptions, yet a certain approximation in the numbers cannot be denied, extending apparently as far as the 8th magnitude, if we assume the numbers of the Durchmusterung, augmented proportionally for application to the entire firmament. For the higher magnitudes the formula gives numbers even more glaringly in defect than those derived from the Durchmusterung. And finally a comparison of the numbers in the northern hemisphere, taken from Heis, and those of the southern hemisphere, derived from the Uranometria Argentina, shows a decided excess of stars in the northern sky, a fact which is itself at variance with our assumption of an approximately equable distribution.

Yet although the accordance of the observed magnitudes with any series of numbers in geometrical progress is far from being close enough to warrant any important inferences regarding the total number of stars of any magnitudes outside of the limits of observation, it affords the only means at our disposal for any of those rough estimates which are needed in cosmological inquiries regarding these numbers. As soon as we have extended our researches to that distance at which the agglomeration of stars in the Milky Way begins to be appreciable, all further inquiries of this character are futile, and it becomes necessary to consider the Galaxy by itself. From a comparison of the geometric series with the numbers observed for stars not beyond the $8\frac{1}{2}$ magnitude, Littrow has inferred that the same formula may be applied to regions considerably beyond this limit, and from the consideration of W. Herschel's star-gauges in the poorest regions of the sky, he fixes upon a magnitude not far from $11\frac{1}{2}$ as indicating the outer limit of this equable distribution, and thus assigns $4\frac{1}{2}$ millions as the probable number of stars within this limit. To me neither premise appears very tenable. If the influence of the Milky Way is not appreciable even for stars of the 9th magnitude, then the number of stars at a less distance than the limit of galactic agglomeration is not even approximately conformable to geometric progression; so that these inferences from the star-gauges must be illusory. The rapid increase in the number of stars after passing the 9th magnitude may be partially accounted for by the

difficulty of estimating magnitudes correctly, near the inferior limit of brightness; yet very large allowances for over-estimation of the fainter magnitudes fail to permit any satisfactory representation by a geometric progression. It is impossible to represent even approximately the more than 150,000 stars of the 9th magnitude, together with the numbers for magnitudes below $4\frac{1}{2}$, by any such formula, without over-representing the intermediate numbers by more than one-half. And even then, we fail to obtain an adequate value for the stars beyond 9 and $9\frac{1}{2}$. We may reproduce the numbers which the Uranometries give for the 6th magnitude and the Durchmusterung for the 9th, by means of a series which doubles the number of stars for each successive half-magnitude; yet even this, when extended to stars of the $9\frac{1}{2}$ magnitude, would assign to the entire heavens a less number than the Durchmusterung gives for the northern hemisphere alone. I attribute, however, to this last consideration comparatively little weight, for the reason already indicated.

In the second column of the annexed table, the number of northern stars corresponding to each half-magnitude is reproduced, being taken from Heis's Uranometry for magnitudes up to the 6th inclusive, and from the Durchmusterung for higher ones. The last line, however, is not for the full half-magnitude corresponding to $9\frac{1}{2}$, but merely for 9·3, 9·4 and 9·5; the remaining two, 9·6 and 9·7, not being contained in Argelander's work. The third column contains the numbers from the Uranometria Argentina, and the fourth the corresponding ones for the entire heavens, being the sum of the two preceding as far as the 7th magnitude, and thereafter the double of the numbers observed in the northern hemisphere. The final column shows, for the sake of comparison, the values which result from the simple hypothesis that the number of stars doubles for each successive half-magnitude applied to the observed number of the 6th magnitude. Simple as it is, this series presents less violent discordances than any of the others which I have deduced, for magnitudes above the 4th. For stars brighter than these, the discordances, although relatively enormous, are intrinsically small. The total number of all stars to the 4th magnitude inclusive is six hundred and sixty-four according to direct observation, but would be only three hundred and five by the empirical series. And the theory of an approximately

equable distribution for stars as bright as the 9th inclusive thus appears less improbable, if we may suppose some three hundred and sixty additional stars to be situated in our immediate vicinity.

TOTAL NUMBER OF STARS.

Mag.	N.	S.	Total.	Formula.
1	8	6	14	4
1½	7	4	11	5
2	35	20	45	10
2½	25	33	68	10
3	55	41	96	38
3½	103	87	190	76
4	132	108	240	153
4½	254	154	408	306
5	392	240	632	612
5½	696	563	1259	1224
6	1374	1075	2449	2449
6½	2231	2022	4253	4898
7	4608	3317	7925	9796
7½	6878	....	13756	19592
8	14525	....	29050	39184
8½	28486	....	56972	78368
9	78185	....	156370	156736
	177505	....	355010	161660

In this connection I desire to mention a fact which early attracted and repeatedly compelled my attention during my residence in South America. It has generally been assumed that the number of visible stars of any given magnitude—whether brighter or fainter—diminishes as their distance from the Milky Way increases. In the elevated position and pure atmosphere of Cordoba, this nebulous circle is seen with a vividness far surpassing that to which we are accustomed here, and moreover most of that portion which lies in the southern hemisphere is intrinsically brighter than the northern half; so that its position is far more clearly defined than I have ever seen it elsewhere. And few celestial phenomena are more palpable there than the existence of

a stream or belt of bright stars, including *Canopus*, *Sirius* and *Aldebaran*, together with the most brilliant ones in *Carina*, *Puppis*, *Columba*, *Canis Major*, *Orion*, &c., and skirting the Milky Way on its preceding side. When the opposite half of the Galaxy came into view, it was almost equally manifest that the same is true there also, the bright stars likewise fringing it on the preceding side, and forming a stream which, diverging from the Milky Way at the stars *Alpha* and *Beta Centauri*, comprises the constellation *Lupus* and a great part of *Scorpio*, and extends onward through *Ophiuchus* toward *Lyra*. Thus a great circle or zone of bright stars seems to gird the sky, intersecting with the Milky Way at the Southern Cross, and manifest at all seasons, although far more conspicuous upon the Orion side than on the other. Upon my return to the North, I sought immediately for the northern place of intersection; and although the phenomenon is by far less clearly perceptible in this hemisphere, I found no difficulty in recognizing the node in the constellation *Cassiopea*, which is diametrically opposite to *Crux*. Indeed it is easy to fix the right ascension of the northern node at about 0h. 50m., and that of the southern one at 12h. 50m.; the declination being in each case about 60°, so that these nodes are very close to the points at which the Milky Way approaches most nearly to the poles. The inclination of this stream to the Milky Way is about 25°, Orion occupying a position midway between the nodes.

A considerable portion of the bright stars of our firmament is situated within this zone or stream, or in its immediate vicinity. It has been a source of surprise to me that it had not previously attracted the notice of astronomers, and since writing the foregoing paragraphs I had begun the preparation of some data in statistical form to demonstrate its existence, when I discovered that it had been alluded to by Sir J. Herschel in his Results of Observations at the Cape of Good Hope. His words are as follows (p. 385):

"It is .... in the interval between η *Argus* and α *Crucis* that the galactic circle, or medial line of the Milky Way, may be considered as crossed by that of the zone of large stars which is marked out by the brilliant constellation of *Orion*, the bright stars of *Canis Major* and almost all the more conspicuous stars of *Argo*, the *Cross*, the *Centaur*, *Lupus* and *Scorpio*. A great circle passing through ε *Orionis* and α *Crucis* will mark out the axis of the

zone in question, whose inclination to the galactic circle is therefore about 20°, and whose appearance would lead us to suspect that our nearest neighbors in the sidereal system (if really such) form part of a subordinate sheet or stratum, deviating to that extent from the general mass which seen projected on the heavens forms the Milky Way."

Yet he does not appear to have recognized the fact that this zone of bright stars may be traced with tolerable distinctness through the entire circuit of the heavens, forming a great circle as well defined as that of the galaxy itself.

These stars, or, more strictly speaking, this excess of stars, in the regions in question, must be deducted from our total before the remainder of the fixed stars can legitimately be subjected to statistical discussion with a view to determining the law of their distribution, and the distance at which the agglomeration which we recognize in the Milky Way begins to make itself manifest. Then we shall perceive that the inferences from the accurate estimates in the Uranometries and the rough ones of the Durchmusterung are by no means discordant, and that the distribution of the fixed stars, up to the 9th magnitude inclusive, is not merely tolerably uniform but approximately such that the number of stars doubles for each successive half-magnitude. The distance corresponding to the 9th magnitude is from thirty-two to forty times that of the faintest first-magnitude star; and the light-ratio between stars differing by a single magnitude becomes 0·3968. This is very close to the ratio 0·4, which photometric researches have seemed to indicate as best expressing the actually existing scale, and which is the value usually accepted. Did we adopt precisely this ratio, we should find 315 as the total number of stars as bright as the 4th magnitude, being only ten more than was given by the value just considered, while the computed numbers for all other magnitudes below the 9th would be brought somewhat nearer to the observed ones, and for the 9th magnitude itself would be 151,260.

The phenomena and numerical relations to which I have referred in this paper seem of considerable importance in their bearing upon the position of our sun in its cluster, the form of that cluster, and the scale of distances between its constituent stars.

ON THE TAILS OF COMETS. By HENRY M. PARKHURST, of New York, N. Y.

MIDWAY between Canopus and the Southern Cross, is still visible, scarcely to be discerned by the naked eye, the Comet (III: 1874) which has been for the last two months my careful study, and which will be my chief topic in this paper. Erroneous as were the orbits at first attributed to it, few things are better established than the theory by which we may exactly predict the motion of the nucleus of a comet; and it is easy to determine its position long after it has ceased to be visible. It is time that the prediction of the form and appearance of a comet's tail should be brought within the domain of science.

The length of a comet's tail, which is the first thing usually noted, will probably be the last to be safely predicted, because there is always a portion which is invisible from diffusion, and its visible length depends, therefore, chiefly upon its brightness. The increase of the actual length of the visible tail, estimated at one-twelfth each day, although sufficiently accordant with the observations of Donati's and of Coggia's comets, whose perihelion distances did not much differ, would apply to few other comets, and especially would not apply to comets like those of 1680 and of 1843.

In predicting the form and position of a comet's tail, I adopt the theory of Prof. Peirce:

"Each particle of the matter which composes the tail, is supposed to move in a hyperbolic orbit, with the sun in the focus of the opposite branch, under the influence of a repulsive force emanating from the sun, and decreasing by the law of the inverse square of the distance." ["Gould's Astronomical Journal," Vol. V, page 186."]

The application of the formulæ gives for any required time, one line only, commencing at the comet and extending indefinitely. Prof. Peirce adopted in his computations upon the tail of Donati's Comet, ["Gould's Astronomical Journal," Vol. VI, page 51,] an excess of repulsive force one and one-half times that of gravitation, making his comparisons with the front edge of the tail, and arbitrarily adopting such a repulsive force as best to explain the observations. For the purpose of comparing with the centre of the tail, I adopted in my computations a repulsive

force exactly equal to and replacing gravitation, perhaps by a reversion of polarity, as the more probable law. On comparison with the observations, however, I find that the computed line closely agrees in nearly every instance with the observed right hand edge of the tail; and I shall therefore adopt that as the standard of comparison. Although the comet apparently moved in the other direction, yet in fact that was the front edge of the tail.

Most of the earlier observations were furnished to me by W. S. Gilman, Jr., of New York City, and all the later ones by Lewis Swift, of Rochester, whose observations continued several days after the comet had ceased to be visible in New York City.

From June 12 to June 30, I have only the record of the direction of the tail, without reference to stars [G.] agreeing with the computation. On July 1, the tail "pointed to and reached 55 of Camelus" [P.] The computed line passes nearly through that star. On July 7, the front edge was seen a little to the left of 63 Arg. 749 [Bonn Catalogue]. [P.] The computed line passes a small fraction of a degree to the left of the star. On July 13, the last day when the head of the comet was favorably seen, "the right edge just touched o Ursa Majoris, as seen by Mr. Swift, and passed $\frac{1}{2}°$ to the left of it according to my own record. The computed line passes $\frac{3}{4}°$ to the left of it. The curvature on this evening was commonly noticed. Computation indicates an arc of about 15°. On July 14, "h U. Maj. was in the middle of the tail." [S.] Allowing for the recorded width, it should be in the middle of the tail. On July 17, "v was exactly in the centre," and "its left edge just touched α." [S.] These are each within $\frac{1}{2}°$. On July 18, "it passed over α," [S.] and α U. Maj. was within the range of the computed tail. "ϑ and F [φ] were not only in the centre of the tail but in line with its axis." [S.] They were within $\frac{1}{4}°$ of the computed centre, and very nearly in the computed line of its axis at that point. On July 19, "the tail passed midway between α and β, but touched neither." [S.] The computed line passes nearly centrally between them. "Its left edge just touched δ." [S.] Here is an inaccuracy; for it could not have touched δ without passing over β. On July 20, it passed "midway between β and γ U. Maj. and centrally over δ." [S.] This agrees with computation. On July 21, the last day of its visibility, γ U. Maj. was seen "at its right edge, per-

haps 1° within it." [S.] The computed line is 1° to the right of γ. "λ and χ U. Maj. were seen in the centre of luminosity." [S.] This agrees exactly with computation. "No curvature perceptible." Computation indicates that the tail increased its curvature until about the 13th, a few days earlier the tail being too short for it to be perceptible, and a few days later the curvature itself becoming too small to be perceptible.

In all these cases the accordance of the right hand edge of the tail with the computed line was as close as the nature of the observation would admit. But among the observations kindly furnished me by Mr. Gilman, the accuracy and faithfulness of which I cannot doubt, are two which I cannot in fairness omit. On July 3, he recorded the place of the tail as wholly to the right of the two stars 65 Arg. 606, 607. The computed line passes to the left of those stars. As he did not use a diagonal prism, and inverted his chart in making the comparison, it seems impossible that he should have put it upon the wrong side, especially as his attention was also directed to the star 65 Arg. 599 near the other edge. On July 6, he recorded the place of the tail as passing through the star 63 Arg. 749, the chief star of an unmistakable group. The computed line passes nearly a degree farther east. On this diagram you will see that the observed lines, before and after, are nearly parallel, and that these two widely deviate. You will also see, that in each case, connecting the observed point in the tail with the position of the head on the previous evening, produces a line parallel to the rest; and although Mr. Gilman himself does not conceive it to be possible that he connected the position of the tail on one day with that of the head upon another, or that he could have even seen the comet on July 5, which was Sunday, I can find no other satisfactory solution of the discrepancy; and that explanation would substitute two observations accurately agreeing with the computations, for two which are entirely inconsistent with the remainder of the series. Perhaps I should add that these observations, and indeed all the observations but one, were made without any expectation that they would ever be of value.

In predicting the width of a comet's tail, it may be assumed to bear a certain ratio to its length. Up to the time of the disappearance of the nucleus, the ratio of one-sixth was sufficiently exact. The tail was then approaching us, and on this account would be

expected to appear to grow wider as well as longer. Assuming that it remained unchanged in form, it should have been about six times as wide on July 21, as on July 13, but instead of being 18° wide it was seen to be only "$4\frac{1}{2}$°, possibly 5°."

I have refrained from alluding to an important point with regard to the front edge. When the tail is coming directly towards us, the front edge becomes the medial line. When the tail is 4° wide, assuming the right hand edge to be the front edge would involve an error of 2°; or, if the tail were 18° wide, as computed, it would involve an error of 9°. There is no such error; therefore there is no visible portion of the tail to the right hand of the true front edge. If the tail is flat, lying in the plane of its orbit, we may readily understand why it should be so foreshortened when directed towards us. Indeed, this proves too much; for if it were as thin as Saturn's ring, it should have appeared as a mere line instead of being 5° wide. If the form of the section were elliptical, it would account for the observed width on the 21st of July; but when the tail was first seen, the earth was only 11° above its plane, as seen from the comet, and the tail would have appeared much narrower in all the observations before July 13. The dark line behind the nucleus cannot indicate a hollow conical tail, for a diminution of the central light by $\frac{1}{26}$ part would hardly be perceptible; but that dark line, and the dark lines separating the different envelopes, are perfectly consistent with the theory of a thin flat tail.

There is another important fact. The front edge was towards the right until July 20; but at noon of July 21, the earth passed through the plane of the orbit. On the evening of July 21, therefore, when the tail was 5° wide, the front edge should have been either at the left hand, or at any rate, at least 2° from the right hand edge; and yet the latter agreed with the line computed from the same formulæ with those of all the preceding days; and although the tail was seen for five days in succession, during which the earth passed through the plane of the orbit, there is no apparent discrepancy between the computed line and the observed position of the *right hand* edge of the tail.

This diagram illustrates several results necessarily depending upon the hyperbolic theory. The black line represents the orbit of the comet of 1843, so far as included within the radius of the earth's orbit. The red lines, starting from the orbit, and after

passing their perihelion points radiating in nearly straight lines from the sun, represent the hyperbolic orbits of particles of matter leaving the head of the comet at the given times. In these computations, also, I have adopted 1 as the ratio of repulsion. Had Coggia's comet possessed sufficient luminosity to be visible on July 24, it would have afforded a test of the true amount of repulsive force; but as its plane was still turned almost directly towards us when the tail was last seen, a variation in the assumed amount of the repulsive force makes no appreciable difference in its position; and the streamers, seen extending from the convex side of the tail of Donati's comet, suggest the theory that upon every formation of a new envelope, a small portion of the matter was endowed with a repulsion at least ten times that of the rest of the matter forming the tail.

The blue lines circling around the sun connect the positions of the particles in their hyperbolic orbits at given times, and therefore represent the visible and the invisible tail of the comet. The particles leaving the head of the comet thirty days before it reaches its perihelion, follow nearly behind it, with continually retarded motion, and when the head of the comet reaches the sun, those particles have only passed over half of that distance, and are just about to commence their outward course. In receding from the sun, the motion is gradually accelerated until the particles reach the same distance from the sun at which they left the head. The moment they pass outside of that point they are irrevocably lost. A reconversion of the repulsive force into attraction would not avail, but they must fly, unless intercepted, beyond the confines of our solar system.

Particles leaving the head of the comet at a later date pursue a somewhat similar course, coming nearer to the sun, but all at about the same time turning and flying off into space. The nearer they come to the sun before commencing their hyperbolic course, the more violent is the repulsion and the greater their outward velocity. The particles which leave the head of the comet exactly at the perihelion, pursue a line almost directly from the sun, but about 66° from the axis of the comet's orbit. These particles commence their outward course with a velocity sufficient to carry them outside the earth's orbit in a little over two days; and that velocity remains nearly uniform in consequence of the sudden removal of the matter beyond the sphere of the sun's for

cible action. Consequently, when we undertake, for the thirtieth day after the perihelion passage, to trace the entire tail which has left the head during the preceding sixty days, we must pursue a curve commencing at the position of the head of the comet, passing to the right and downward, crossing the perihelion line at a distance from the sun one-third greater than that of the planet Neptune, and reëntering the earth's orbit above.

How much of this will be visible? As we follow the tail from the head, it becomes fainter, first, because it is more distant from the sun, and therefore less illuminated. Another reason is that it is more diffused from its rapid expansion like the fire from a Catharine wheel, the portion of the tail forming a certain arc, continually forming nearly the same arc of a larger circle. But is that all? Here is a portion of the tail above, more illuminated than the head of the comet itself, and which occupies less than one-fourth the space travelled over by the comet while it was issuing.

While the dark line in the centre of a comet's tail seems to disprove the theory naturally suggested by the fact of the front edge being the line of computation, that the resistance of the ether is the cause of the expansion of the tail, the ether may have a perceptible effect upon its form and position. Had the sun an ether of its own, carried with it through that still more rare ether which brings to us the pulsations of light, the effect of the ether upon the tail would be only that resulting from the motion of the comet itself; but leaving the ether behind it in its course towards the constellation Hercules, it will tend to sweep the tail back from that point; and here we may find a possible explanation of the apparent wafting of the tail out of the plane of its orbit, leaving the front edge still on the right, and causing a thin flat tail to have a visible width tenfold greater than it would have had if strictly in the plane of the orbit.

[*Note.* Observations in England on July 9, 14, 17, 18, 19, and 21, confirm the positions above given, but are not sufficiently definite for exact comparison. The theory of Prof. Norton (Am. Jour. Sc., 1861) that the width of the tail arises from the variation of the repulsive force exercised upon different particles of the cometary matter, and its thickness from the repulsion of the nucleus itself at the time of emission, I had not seen when the above was written. I have computed a new line consisting of particles equally repelled and attracted, and I find that it nearly corresponds with the former computed line, or with the observed right hand edge of the tail, apparently indicating that the tail was, from some cause, chiefly south of the plane of the orbit. The theory of Prof. Norton affords a plausible explanation of that cause. The careful series of observations by Prof. Cleveland Abbe, in Washington, I did not hear of until they were published in the Astronomische Nachrichten, No. 2015, in November, and they were received too late for even a hasty review.]

New York, Dec., 1874.

On the Loss of Light in its Transmission through Space.

By Henry M. Parkhurst, of New York, N. Y.

The general facts of the constitution of the universe, revealed by the telescope, suggest the idea of an infinite extent of space, filled with stars and nebulæ, arranged in systems, sometimes so vast as to amount to galaxies, and all subject to the same laws. Let us see what this idea necessarily involves.

Let us first assume that all the stars are of equal size, of equal intrinsic brightness, and scattered throughout space at equal distances. With the sun as a centre let all space be divided into concentric shells, the outer surface of each being at $\sqrt{2{\cdot}512}$, or 1·585 times the distance of its interior surface. Each shell will then consist of stars one order of magnitude higher[1] than that next interior. The number of stars in each shell will be $1{\cdot}585^3$ or 3·981 times that in the next interior shell. The aggregate brightness of the stars of any order of magnitude will then be $3{\cdot}981 \div 2{\cdot}512 = 1{\cdot}585$ times that of the stars of the next lower[1] order. It would necessarily follow that the heavens would be one blaze of light, there being an infinite ascending series. Now assuming that the stars are of unequal size, of unequal intrinsic brightness, and at unequal distances, but scattered throughout space, we reach the same result as soon as we pass to a sufficient distance for the law of averages to apply.

It follows therefore that the stars are exceedingly concentrated in the region of the solar system, and that the spaces beyond are nearly or quite empty, unless there is a loss of light in its transmission through space.

Let us next assume that this vast aggregation of stars is only one of an infinite number, that the nebulæ are, some of them, glimpses of similar aggregations, separated by vast intervals of empty space from our own system. We have only to apply the same reasoning, extending our conception to a point where myriads of such clusters of stars would be contained within a single concentric shell, to demonstrate that upon that assumption the heavens would still be one blaze of light. And should we attempt to meet the difficulty by supposing aggregations of such aggrega-

[1] Looking towards the zenith, the more distant stars will be higher; and this mode of expression assigns the larger numbers to the higher magnitudes.

tions, to any compound order, it all comes back to the same simple proposition, that whatever systems of stars or of aggregations of stars there may be, luminous matter is exceedingly concentrated in the region of the solar system, the space beyond being comparatively empty, unless there is a loss of light in its transmission through space.

The lost light cannot be intercepted by dark bodies; for there must be enough to cover ·369 of the entire heavens in order to produce an extinction of ·5^{m}, which, assuming these dark bodies to subtend a visual angle of 1″, at the distance of 6^{m} stars, would be 155,000,000,000 large dark stars nearer to us than the stars of the 6^{m}; at least a million times more than can reasonably be supposed to exist, and a number which could not exist without producing occultations innumerable.

Another evidence of the loss of light in space is found in the comparative numbers of the stars of the different magnitudes. Uniform distribution would place in each shell nearly four times the number in the next interior shell.

Assuming that the stars are of unequal size, the ratio of numbers of the nearer stars, especially if the smaller stars are the more numerous, as is almost certainly the fact, must be more than four. The actual number of each magnitude, as I have ascertained from photometric observations, assisted by the catalogues, is only three times that of the next lower magnitude, the total light of the stars of each magnitude, from the 4th to the 10th, being approximately equal. From the 10th to the 13th I consider the ratio still less; but, in the absence of catalogues of these stars, I cannot state it accurately. Such a mathematical concentration of stars towards the earth as a centre, as any ratio less than four would indicate, is so improbable as of itself to intimate strongly a regular diminution of the light received by us from the stars, in proportion to their distance.

The ascertainment of the amount of the extinction of light depends upon two quantities as yet not well determined; the number of stars of each magnitude, of a known ratio of brightness; and the diffused light from stars too distant for resolution. The ratio of magnitude I assume to be 2·512. The comparative numbers I derive from catalogues, reducing the numbers to the assumed ratio, and by extensive counting of the stars in different regions, with

different apertures and different magnifying powers. The light of the sky I derive from direct photometric observations. With these data, and an assumed extinction of light amounting to $\cdot 05^m$ in passing a distance equal to that of a 1^m star (that is, of about the brightness of Altair), I have formed the following table :—

Mag.	Light.	Dist. or Tot. Light.	Number.	Ap. Mag.
1	1·0000	1·000	1·335	1·050
2	·3981	1·585	4·000	2·079
3	·1585	2·512	16·000	3·126
4	·06310	3·981	63·000	4·199
5	·02512	6·310	251·000	5·315
6	·01000	10·000	1000·000	6·500
7	·003981	15·85	3981·000	7·792
8	·001585	25·12	15850·000	9·256
9	·0006310	39·81	63100·000	10·990
10	·0002512	63·10	251200·000	13·155

Column 1 is the magnitude of stars, assuming on the average uniformity of distribution in space and of intrinsic light.

Column 2 is the relative brilliancy, with the ratio 2·512, no deduction being made for loss of light.

Column 3 is the relative distance. Substituting 1·335 for 1·00, it also represents the total light of the stars of each magnitude, all the stars in each shell being assumed to be on its outer limit.

Column 4 is the relative number of stars of each magnitude. Substituting 1·00 for 1·335, it also represents the relative number including all brighter stars, the unit now referring to the cubic contents of the sphere instead of the shell.

Column 5 is the apparent magnitude, as reduced by extinction, in proportion to the distance.

Making the argument the apparent magnitude, with the assumed extinction of light, I have formed the following table:

Mag.	Distance.	Number.	Tot. Light.	Actual Number.	Tot. Light.
1	·978	1	·935	1	1·0
2	1·530	3	1·054	3	1·2
3	2·378	10	1·564	9	1·5
4	3·659	36	2·243	29	1·8
5	5·552	122	3·067	96	2·4
6	8·267	394	3·939	325	3·2
7	12·018	1171	4·662	945	3·8
8	16·988	3166	5·018	2920	4·6
9	23·288	7728	4·877	11090	7·0
10	30·944	17000	4·270	14500	3·7
11	39·901	33900	3·390	44200	4·4
12	50·055	61870	2·464	94200	3·7
13	61·273	104650	1·659	239000	3·8
			Sky. 2·418		Sky. 2·3

Column 1 is the apparent magnitude. The relative brilliancy is given in Column 2 of the preceding table.

Column 2 is the relative distance.

Column 3 is the relative number of stars of each magnitude.

Column 4 is the total light of the stars of each magnitude.

Column 5 is the actual relative number of stars.

Column 6 is the actual total light of the stars of each magnitude.

In the last line is the actual light of the sky. It is derived from observation, and includes all the light visible after twilight ends; and therefore should be in excess of the truth.

From the last table it is apparent that the assumed extinction exceeds the truth; first, because the number of stars is a little less than the actual number; and second, because the maximum total light is reached too soon, is too low, and diminishes too rapidly. Yet it is also evident that it does not far exceed the truth, because the number of stars and the total light correspond much more nearly with this supposition than with the theory that

there is no extinction; and the brightness of the sky, which would be infinite if there were no extinction, does not vary materially from what the theory requires. A more accurate approximation might easily be made, but seems hardly desirable until the last two columns shall have been corrected by a larger number of observations, and extended by observations with a larger telescope than my own.

The first argument is answered by the theory founded upon the mathematical principle that the sum of a decreasing geometric series is finite, because to that theory the law of averages cannot apply; but the second argument, founded upon the observed numbers of stars, is unaffected by that theory.

EXPERIMENTS TO DETERMINE THE LIMIT OF PERCEPTION OF MUSICAL TONES BY THE HUMAN EAR. By LAURENCE TURNBULL, of Philadelphia, Penn.

ABSTRACT.[1]

BEFORE giving the writer's experiments, it will be proper to state the results obtained by other observers. The discrepancy, in our opinion, can only be accounted for in three ways. First, variations in the sound conducting portion of the auditory apparatus; second, defect, original or acquired, in the perceptive portion, and third, want of cultivation.

Savart fixed the lowest limit of the human ear at eight complete vibrations a second, by means of a toothed wheel and an associated counter,. and the highest limit at twenty-four thousand vibrations. Helmholtz has fixed the lowest limit at sixteen vibrations and the highest at thirty-eight thousand. Vierordt gives the highest tone as forty-eight thousand, and Desprets as seventy-

[1] Of Paper.

three thousand seven hundred single vibrations in the second. By means of a pendulum swinging a given distance and striking a steel König rod, a definite degree of intensity is obtained. With this instrument a series of experiments were made by Dr. C. J. Blake of Boston, Mass., to ascertain the average perceptive power in the normal ear, and this was found to vary considerably with the age; thus at about the ages of twelve and thirteen years a tone of forty thousand, nine hundred and sixty vibrations per second was heard at a distance of thirty-four feet; at the ages of from eighteen to twenty years, the same tone was heard only at distances of from thirteen to sixteen feet; and at the extreme limit of thirty-four feet, the tone of thirty-six thousand, eight hundred and sixty-four vibrations per second only; at the ages of from twenty-eight to thirty years, at the extreme limit of thirty-four feet, only tones up to thirty-two thousand, seven hundred and sixty-eight vibrations per second were perceptible; while above the age of fifty years the limit of perception at the same distance had still further diminished, and in a greater variety of degree.

In the writer's experiments for the production of the musical tones, the steel rods of Dr König of Paris were employed, made of choice white tempered steel, under the supervision of Dr. Clarence J. Blake, M.D., of Boston, Mass.

The rods are two centimetres in diameter, and in length from one inch to four inches, yielding from twenty thousand to sixty thousand vibrations per second. They are suspended at the nodes by means of fine wire, or strong silk thread: vibration is communicated by a stroke from a steel hammer.

In each experiment the rod is held at a uniform distance from the ear to be tested. Before commencing our experiments on any individual we determined accurately the condition of his organ of hearing. The testing of the hearing power was, first, ordinary speech, syllables, words or sentences repeated in three grades or intensity of tone, loud, medium or conversational, and whisper; second, the tick of a watch (the intensity of which had been previously determined); third, the tuning fork; fourth, the musical box and piano; fifth, examination of auditory canal and membrana tympani by means of the aural speculum and mirror, with both direct and reflected light, natural and artificial.

Barometric pressure 30° and temperature 65° F. With this arrangement of the rods the following were the results obtained of

a series of experiments which were found to vary with the age and condition of the hearing apparatus of the individual.

Age.	Distance.	No. of Vibrations.
15	35 feet	40,000
18	" "	40,000
21	" "	35,000
25	" "	30,000
30	" "	25,000
50	" "	25,000
60	" "	20,000

In only one instance in a trained musical ear were sixty thousand vibrations in a second heard.

It will be seen by these and numerous experiments which have been made, that the highest musical tone obtained in a normal healthy ear, not specially trained, did not exceed in any instance a fraction over forty thousand vibrations in a second. Education of the ear and brain, to the recognition of musical tones, is as necessary as the training of the athlete to run, box, row, bat or lift weights. Most wonderful instances of the power of the trained ear are found on record. There are some drawbacks in these experiments with high tones, for in some recent experiments of a friend with Dr. König while in Paris, he found that he suffered from a painful tinnitus or ringing noise in the ears, after he had reached a limit of forty thousand vibrations per second; and this he found was also the case with Dr. König. To certain individuals' ears these high tones are very painful. In every instance when the individual experimented upon was fifty years of age or over, there was found a change in the auditory, or sound conducting apparatus, and even in the case of individuals who were from eighteen to thirty years old it was rare to find both ears of the same power.

In other instances where there was no defect in the ear there was found a want of musical perception.

A gentleman[2] who has given much study of sound, throat, vocal cords, etc., was desired by the writer to repeat his experiments with the rods which he kindly complied with. On returning them he stated that he could not hear a higher number than thirty-five thousand vibrations. I stated at once that there must be some

[2] Dr. J. Solis Cohen, of Philadelphia.

defect, and expressed a wish to examine and test his hearing, never having done so before and without knowing of any peculiarity in regard to his hearing. On examination I discovered that in the right ear there was an opacity or thickening on the inferior quadrant of the membrana tympani, and this was confirmed by testing his hearing by the watch (of thirty inches normal hearing), when I found his right ear was less acute in hearing by four inches. He then wrote for me the following note of his case, a copy of which I append.

"I have recognized for a long time in delicate auscultations that hearing in my left ear is little more acute than in my right, but I can hear the upper tones in the rod of thirty-five thousand vibrations, apparently as well with one ear as the other, the musical tone being distinct though faint. In the rods of forty thousand vibrations and upwards I can distinguish nothing but the thud. For ordinary purposes I am not conscious of any difference in hearing capacity, nor aware that my hearing is less acute than that of the average normal subject."

Sudden Fluctuations of Level in Quiet Waters—Records of Observations. By Charles Whittlesey, of Cleveland, Ohio.

I have thought it would be of service to future observers, and students, on the subject of those fitful fluctuations, which occur in all waters, without visible cause, to publish such statements as are in my memoranda, nearly in their original form. In some cases they are somewhat compressed, but the substance is retained. Prominent swashes, or as the Italians have it "Seiches," are generally followed by a storm.

LAKE SUPERIOR.

(*a*) In 1789 Alexander Mackenzie was at the Grand Portage on the north shore opposite Isle Royal; and states that "there

was a sudden fall of the water, equal to *four* feet, which soon returned with a rush, and continued to vibrate several hours."

(*b*) Whitney and Foster's Geol. Report Upper Peninsula of Michigan, pages 48–53. In the summer of 1834, the water above the falls, at the Sault St. Mary, in a calm day, suddenly fell two and one-half feet, and in half an hour came surging back with great velocity.

The same thing happened, at the same place, in 1842, below the falls; the current of the river rushing rapidly up stream, towards the cataract.

(*c*) Dr. Foster's Statement. "In August, 1845, coasting in an open boat, between Copper Harbor, and Eagle River, we observed the water to rise up a fourth of a mile to the northwest, to the height of twenty feet. It curled over like an immense surge, crested with foam, and swept towards the shore diminishing as it advanced. The voyageurs paused on their oars, having first headed the boat so as to cut the advancing wave. It passed without doing us any injury, succeeded by two or three swells, spent its force on the shore, and the Lake resumed its wonted tranquillity."

The Lake was calm, with clouds at the northwest, indicating that different currents of air were moving in opposite directions.

On the same page, are other observations of Dr. Foster, upon the sudden flow and reflux of the waters in 1847–8–9, which preceded or followed storms on the Lake.

(*d*) In November, 1851, at Copper Harbor according to Mr. D. D. Brockway, the water rose suddenly, in a perfect calm one foot three inches, at another time two and one-half feet.

(*e*) *Lake Superior News*—Sault St. Mary, July 17, 1855. "About 9 A. M. of the 16th the water rose rapidly in the canal, eight inches, light breeze, northwest. In ten minutes fell fourteen inches, the depth of water on the mitre sill being then thirteen feet. At 11, A. M. it had fallen to ten feet nine inches, in all *two feet two inches*, rising and falling until 4 o'clock, P. M., when it resumed its previous level. The wind changed during this period, from northwest to southeast."

(*f*) *Superior City Times*—Sept. 22, 1865. *Ebb and flow of the waters.* "Between half-past 12 and 1 o'clock, P. M., Sept. 17th, by our tide gauge, the water of the lake fell twenty-five inches, and within fifteen minutes rose twenty-five inches above the pre-

vious level, making *fifty inches change* in fifteen minutes—wind strong from northwest."

LAKE MICHIGAN.

(*a*) When Father Andrè was on Green Bay near the Menominee River, in 1670, he says, "the weather being calm, we left our canoe afloat, and on the following morning found it on dry ground, at which we were the more surprised, because the lake had long been tranquil. The ordinary rise is one foot and the highest *three feet*, but this was accompanied by a northwester."

(*b*) *Milwaukee Sentinel*—Apr. 14, 1858. "Shortly before noon on Thursday last, a wave came rushing into the river from the lake so strong, as to upset the Ferry Boat on the Menominee.

The water which, just before the fall, was within three feet of the warehouse floor at Kellogg and Strong's pier, retired rapidly, leaving dry ground under the building. In about twenty minutes it returned in two waves, rising so as to force up the floor of the warehouse, and flow into the cellars, at the lower end of Huron street. After a few minutes, it returned to the old level. The difference in half or three quarters of an hour was full *six feet*."

LAKE ERIE.

(*a*) Col. Bradstreet's disaster, which occurred on the 19th or 20th of October, 1764, near the place mentioned below by Mr. Taylor, appears to have been due to a sudden rise of the lake, which was followed by a severe storm.

(*b*) Mr. Taylor, an old settler of Rockport, Cuy Co., Ohio, states that in 1811, about three miles west of Rocky River, on the south shore of Lake Erie, during a calm hot summer day, he saw a white crested wave approach rapidly towards the beach. It carried a barrel of salt several rods, over what had been dry ground into a ravine.

(*c*) *Memoirs New York Literary and Philosophical Society, vol.* 2 p. 1. On the 10th of May, 1823, according to De Witt Clinton, at Otter Creek on the Canada shore of Lake Erie, when the water was calm, a wave came in shore, nine feet high, rolling over the beach into the woods. A schooner of thirty-five tons was thrown upon the land. The same thing occurred at the same time at Kettle Creek twenty miles distant.

(*d*) *Black Rock*, Dec. 16, 1867.—A strong gale from the east

caused the waters to recede until that part of Niagara east of the Island was nearly destitute of water. Below Niagara Falls the water fell twenty feet.—*Corr. New York Tribune.*

(*e*) A correspondent of the "Cleveland Herald," in the spring of 1866, writes that "in the spring of 1830, Mr. Luther Winchell, a resident of Lake Co., Ohio, and three others, were fishing at Madison Dock, Lake Erie. The day was pleasant, no wind stirring, the lake entirely smooth, with no wave or ripple. While rolling up their net they were struck by a wave with great violence, throwing them six or eight rods inland.

Mr. Winchell was taken by the swell and borne upon its crest several rods, striking a heavy stick of timber, which lay endwise to the lake, as it was raised by the wave. Running along the timber and jumping off, he found the water over his head. He reached the shore safe, but one of his company was caught under the timber, and materially injured. There were *three waves* or swells, the first rising fifteen or twenty feet. The succeeding ones were less, and the water soon regained its former level.

(*f*) "An old settler relates that in 1844 or 1845, at the mouth of Euclid Creek, Cuyahoga Co., in quiet weather, a wave fifteen feet high, came into the creek suddenly, carrying everything before it."

(*g*) By Col. Stockton's water registers kept at Cleveland, at 4 A.M., Nov. 19, 1845, the water suddenly fell two feet eight inches, high wind from southwest.

(*h*) *Toledo Blade*, Dec. 5, 1856.—"On Tuesday last the water rose here *five feet*, the wind being at the northeast. On Wednesday the wind shifted to southwest, or directly opposite, when the water fell from morning to afternoon as much below the ordinary level, causing a change of *ten feet*.

LAKE ONTARIO.

(*a*) *Rochester Democrat*—June 15, 1872. "On Thursday of this week, several gentlemen and their families were at Charlotte (mouth of Genesee River) for a day of fishing; while taking minnows at the East Pier in about two feet of water, their boat suddenly grounded, and they were surrounded by a sand bank. In a short time the boat was afloat, and dashing about in the water. About twenty minutes after it was aground again. The party now set themselves to watch the tidal wave. For about *ten min*

utes the water receded, then began to rise, and so continued to ebb and flow, till they were driven away by a squall from the east, about half-past 5 P. M. The water rushed over the beach, about five rods above the calm water line, and then retired ten rods below the same line. At the steamboat landing it rose *twenty-two inches*, carrying boats, floodwood, and logs, out into the lake."

SENECA LAKE, NEW YORK.

(*a*) *Geneva Gazette* — May, 1855. "During the whole of Wednesday and yesterday, the waters of Seneca Lake rose and fell, in spaces of time varying from *ten minutes* to half an hour; five inches to *two feet* through both days. A friend of ours made a measurement first after sundown, Wednesday. It fell *sixteen and one-half inches* in fifteen minutes and then rose again."

Similar agitations of the water have been observed on Lake Geneva in Switzerland.

In Peru, according to Von Tschudi, at Callao, in 1841, "About two o'clock, P. M., the sea flowed from the shore with greater force, than in the strongest ebb. Ships farthest out were left dry, which is never the case at ebb tide. The alarm of the inhabitants was great. The sea instantly rushed back with increased force. Nothing could withstand its fury. There was no commotion o the earth nor any marked change in temperature."

TITLES OF COMMUNICATIONS.

The following titles of papers read in Section A include those accepted by the committee for publication in full, but of which the authors have failed to send copy, as well as those which the committee decided should be printed by title only:

Differential Measurements of Solar Temperature. By S. P. Langley, of Allegheny, Penn.

The Distribution of the Poles of Certain Nebulæ. By Cleveland Abbe, of Washington, D.C.

Exhibition of a Combined Collimator and Personal Equation Machine, with a Notice of the Results Obtained with it. By William A. Rogers, of Cambridge, Mass.

Relation between the Barometric Gradient and the Velocity of the Wind. By Wm. Ferrell, of Cambridge, Mass.

On the Molecular Volume of Water of Crystallization. By Frank W. Clarke, of Washington, D. C.

On the Molecular Heat of Similar Compounds. By Frank W. Clarke, of Washington, D. C.

Action of Mechanical Vibration in Retarding Chemical Combination. By S. S. Haldeman, of Columbia, Penn.

Notice of A. K. Eaton's New Compound One-prism Spectroscope. By R. E. Rogers, of Philadelphia, Penn.

On the Proper Motion of Eta-Draconis in Right Ascension. By Wm. A. Rogers, of Cambridge, Mass.

Periodicity of Rates of Interest from Observations for the twenty-five Years, 1849 to 1874. By E. B. Elliott, of Washington, D. C.

Population of the U. S., each Year from 1780 to 1880, with Proof of Estimate and Interpolation. By E. B. Elliott, of Washington, D. C.

Credit of the U. S. Government. By E. B. Elliott, of Washington, D. C.

EXPENDITURES OF THE U. S. GOVERNMENT PER CAPITA TO POPULATION, BY FOUR YEARS' PERIODS FROM THE ORGANIZATION OF THE GOVERNMENT, TO THE 30TH of JUNE, 1874. By E. B. ELLIOTT, of Washington, D. C.

DESCRIPTION OF A NEW MECHANISM FOR PRINTING HOURLY THE DIRECTIONS AND VELOCITY OF THE WINDS. By G. W. HOUGH, of Albany, N. Y.

ON A METHOD OF TRANSMITTING TIME SIGNALS OVER TELEGRAPH WIRES. By G. W. HOUGH, of Albany, N. Y.

SIMPLE FORMULAS FOR SOME OF THE HIGHER CURVES. By JAS. D. WARNER, of Brooklyn, N. Y.

ON THE PRESSURE PRODUCED BY IMPACT. By JOSEPH J. SKINNER, of New Haven, Conn.

ON AN IMPROVED ELECTRIC BELL. By A. A. BRENEMAN, of Lancaster, Penn.

MUTUAL ACTION OF ELEMENTS OF ELECTRIC CURRENTS. By E. B. ELLIOTT, of Washington, D. C.

ON OUTLINES OF THE TYPICAL PHOTO-LITHOGRAPHIC PROCESSES. By J. W. OSBORNE, of Washington, D. C.

THE CHEMISTRY OF STEEL. By B. S. HEDRICK, of Washington, D. C.

ON ABNORMAL PHENOMENA OF SOUND. By JOSEPH HENRY, of Washington, D. C.

ON A NEW FORM OF STEAM GENERATOR, APPLICABLE ESPECIALLY TO VERY SMALL POWERS. By W. P. TROWBRIDGE, of New Haven, Conn.

REPORT ON A REMARKABLE COMPOUND OF PHOSPHORUS AND CARBON, FORMING A NEW PYNOPHORUS. By P. H. VAN DER WEYDE, of New York, N. Y.

INVESTIGATION INTO THE CHEMICAL COMPOSITION OF PETROLEUM. By P. H. VAN DER WEYDE, of New York, N. Y.

THE CHANGE OF PEAT INTO CANNEL OR OTHER COAL BY HEAT AND PRESSURE, PRACTICALLY DEMONSTRATED. By P. H. VAN DER WEYDE, of New York, N. Y.

ESTIMATION OF NITROGEN BY THE ABSOLUTE METHOD. By S. W. JOHNSON, of New Haven, Conn.

Estimation of Nitric Acid by the Methods of Thorpe and Bunsen. By S. W. Johnson, of New Haven, Conn.

On the alleged Formation of Ammonium Nitrite from Water-Vapor and Nitrogen Gas, and on Price's Test. By S. W. Johnson, of New Haven, Conn.

On the Harvard College Observatory System of Communicating Time for Civil Purposes. By Wm. A. Rogers, of Cambridge, Mass.

On the Mineral Warwickite. By J. Lawrence Smith, of Louisville, Ky.

On Metal Iron in Basaltic Rock, and the Dissociation of Oxide of Iron. By J. Lawrence Smith, of Louisville, Ky.

Curious Association of Datholite, Garnet, and Idocrase. By J. Lawrence Smith, of Louisville, Ky.

Exhibition of a new Sulphuretted Hydrogen Apparatus. By H. Carmichael, of Brunswick, Me.

Exhibition of a Platinum Digestor. By H. Carmichael, of Brunswick, Me.

On a Simple Form of Sphygmograph. By A. A. Breneman, of Lancaster, Penn.

Growth of Crystallization in Traps and Slags. By H. Carmichael, of Brunswick, Me.

Some Remarks of Certain German Trap Rocks. By H. Carmichael, of Brunswick, Me.

On the Phosphorescence of Glass, produced by Electricity. By Arthur W. Wright, of New Haven, Conn.

On the Use of Natural Twin Crystals of Quartz in the Construction of Polariscopes. By Arthur W. Wright, of New Haven, Conn.

On the Nature of the Zodiacal Light, and the Distribution of the Matter which Occasions it. By Arthur W. Wright, of New Haven, Conn.

On a Modification of Löwenthal's Method for the Estimation of Tannic Acid. By Wm. McMurtrie, of Washington, D. C.

PART II.

SECTION B,

NATURAL HISTORY.

B. NATURAL HISTORY.

Discovery of Dicotyles (Platygonus) compressus, Le Conte. By John H. Klippart, of Columbus, Ohio.

In the month of April, 1873, workmen were engaged in digging sand on the banks of the Olentangy River, at the crossing of Olentangy and Montgomery streets, in the corporate limits of the city of Columbus, Ohio; and in the course of their labors discovered, embedded in the sand, a "nest" of bones of some animal, to them unknown. A messenger was despatched with a basketful of these bones to my office, when I at once made arrangements to secure the entire lot, and whatever more might be discovered in the course of their excavations. A week later they advised me that they had discovered another "nest," which I secured also—going in person and superintending the removal.

These bones were exceedingly friable; in several instances a tibia broke, or rather crumbled, when made to support very little more than its own weight. The mass of bones were embedded in a calcareous clay, intermingled with a very generous quantity of calcareous sand. After carefully removing the bones from the clay in which they were embedded, they were placed in a kettle containing, say, four gallons of boiling water, in which one pound of the best glue had been dissolved; and kept at the boiling point for the space of one hour after the bones were immersed. Nearly all the bones treated in this manner were restored to a comparatively great degree of firmness and solidity; more especially such ones as were fractured, or in which the internal structure was exposed. But this process—the only one with which I am acquainted for the purpose of restoring the gelatine to the bones—has its disadvantages. I found nearly all the "bases" and "heads" of femora, humeri, ulnæ, and other bones, separated from the shafts; and it was by no means an easy process to replace and unite them again.

Before proceeding to give a detailed description of the bones, it is perhaps, not inappropriate to describe the geological formation

of the locality in which they were found. From the northern boundary of Delaware county until near its junction with the Scioto River, within the city limits of Columbus—a distance of about thirty-five miles—the Olentangy, flowing nearly due south, flows on the outcrop of the Huron shales;—the left bank being the shale outcrop, and often somewhat precipitous. In this shale bank or outcrop, Septaria abound: the extremes in size of these Septaria range from two inches in diameter, or perhaps even less, to more than ten feet in diameter; those from one to two feet in diameter being the most abundant. The right bank of the Olentangy is of "valley drift," and beyond it westward, for many miles, the surface of the country is very level.

About eight miles west of the Olentangy, on the north boundary of Delaware county, is the Scioto River, which flows a little to the east of south so as to unite with the Olentangy in the city of Columbus. The Scioto, however, flows over a bed of corniferous limestone. The valley between these two streams, as well as the country for miles east and west of it, is covered with a deep drift. When boring the artesian well in the State House yard, this drift was found to be one hundred and twenty-three feet thick at that point. But the drift on which the State House and city in its immediate vicinity are built differs very materially from that in which the remains or bones were found. The one hundred and twenty-three feet of material deposited on the fifteen feet of shale, which intervenes between it and the corniferous rock, is material undoubtedly due to the action of glaciers and icebergs. From a record kept at the time of boring the artesian well above mentioned, the auger passed through the following thirteen strata:—

	Ft. from surface.	Ft. in thickn's.	Strata.		Ft. from surface.	Ft. in thickn's.	Strata.
1	1	1	Surface earth	8	42	18	Blue clay
2	3	2	Brown loam	9	45	3	Clay and gravel
3	14	11	Gravel and sand	10	50½	5½	Quicksand
4	18	4	Blue clay	11	54½	4	Clay and gravel
5	20	2	Common sand	12	123	68½	H'd pan, cemented clay, sand & grav'l
6	23	3	Quicksand	13	138	15	Shale and slate
7	24	1	Leafy blue clay and sand				

These strata, or rather deposits, with the exceptions of Nos. 6 and 10, are exceedingly inconstant. Within two squares from the artesian well, No. 3 has diminished on the southward to less than three feet, and 5 and 6 are entirely wanting; on the northward it is less than half as thick as at the well and rests upon yellow clay. The irregularity in thickness of these strata, the disappearance of some in short distances, the appearance of others not in the series at the well, are evidences of the deposits not being subjected to the assorting action of water, and therefore I conclude they were deposited by glaciers and icebergs; and I prefer to ascribe the deposit, at least the lower seventy-five feet, to glacial action. I think there can be little doubt that the valley of the Scioto, that is, that area extending from the left bank of the Olentangy to the right bank of the Scioto, is certainly due to glacial action. At Mr. Little's quarry, in the town of Delaware, about a mile west from the right bank of the Olentangy, and at a slight elevation above the bed of the stream, was found a quartzite bowlder at the terminus of a great groove which it had ploughed; and in so doing had broken into fragments and displaced many of the upper thin strata of the quarry. This quarry is in the upper portion of the corniferous limestone. Erratic, or drift bowlders, are found throughout the material on which the city of Columbus is built — are found beyond the banks of the Scioto as well as in the valley.

There is no doubt that the line of drainage of the Olentangy and Scioto from their entrance into Marion Co., the former on the eastern boundary and the latter on the western, to their junction at Columbus in Franklin Co., is due to glacial action; and that the entire area embraced between them throughout this distance was once a single sheet of water, and whilst in this situation the waters did much erosive work. During the subsequent submersion this "bay" was filled up with eroded material from higher grounds, and was "worked over," *assorted*, and deposited in such strata as we now find it. When it emerged, the old line of drainage was resumed, but as the volume of water had vastly diminished, two small streams were formed, the Olentangy occupying the eastern bank of the former stream, and the Scioto occupying the western. Then came another submergence, and during this period there was deposited a stratum of clay, varying in places from two to five feet in thickness. This last stratum of clay is

found superimposed on the strata of sand in the valley, as well as on the higher table lands, thus presenting indisputable evidence that the clay was deposited after the valley had assumed its present form, so far as its precise topography is concerned. [Diagrams presenting the following details of the locality accompanied this communication but are not reproduced here.—A section taken from the State quarry along the line of the "Shortline" railroad, and above the junction of the Olentangy with the Scioto. Some rods north of the railway and at the junction of Olentangy and Montgomery streets is where the remains were found. Another section was taken diagonally across the corner of the lot from Montgomery street to Olentangy street.]

After having penetrated about twenty feet from the opening of the bank, and at a depth of eight feet from the surface the remains of six of the smallest animals were found, embedded in the calcareous clay and sand; after proceeding some six feet farther and four feet deeper, the remains of the six larger animals were found. The digging proceeded from southeast to northwest; the *snouts*, or rather the tusks, were the first portions of the skeleton which presented themselves. The second group of remains was almost directly under a side track of the railway on Olentangy street, and farther excavation in that direction could not be made without endangering the safety of the track. In order to obtain this last group of remains it was necessary to deviate from the northwest to a due north course, thus presenting a square east and west face of the deposits or formation. This face presented the matter in a new light; it was observed that the strata immediately above the remains were confused, presenting evidence of disturbance so far as the deposits of sand and gravel were concerned, whilst the overlying clay was, so far as the eye could discern, entirely intact or undisturbed.

The animals were lying side by side with their heads towards the southeast; their death was sudden and violent, the jaws were crushed, skulls fractured, the left side of one lower jaw was driven into the palate, and various other evidences of sudden and violent death were manifest. On the contrary there was no evidence whatever that the animals were destroyed by other, or carnivorous animals, and then dragged into the burrow which evidently existed when the animals were placed there. The bones are entire except where broken from very manifest pressure; they present no ap-

pearance of having been gnawed, or *crunched*. In fact a portion which I succeeded in keeping intact, except to remove the earthy matter, proved to be a portion of the spine, with the scapulæ in proper position, humeri, radii and ulnæ; the metacarpal bones all in proper position, and the articulations were apparently not disturbed after the embedding or death of the animal. The evidence—the undisturbed condition of upwards of two feet of superincumbent clay—is to the effect that the animals met their fate and were embedded before the last submergence of this portion of the globe.

That these remains or bones are those of a species of the family Suidæ is evident from the structure of the feet, which in this family have three or four toes, two median, and one or two shorter lateral shod with hoofs, and the lower incisor teeth which are bevelled forwards. A comparison of the fossil skull—although not fossil in the ordinary acceptance of the term, yet fossil because "dug out of the ground"—with that of the domestic hog shows, however, generic differences. The skull is more conoidal as regards both cranium and face, presenting no marked angles in common with that of the hog. The median line is convex, its highest point some distance in front of the occiput. There is no intertemporal space; the temporal ridges meeting to form a crest. The superior portion of the occiput is relatively much smaller than in the hog. There is an interval of nearly two inches between the canines and premolars, the former being shorter, stouter, and less curved than in the hog. The molars and premolars are arranged in straight lines, those on opposite sides parallel, the palate is very narrow, an inch only in width between the teeth. In front of the superior canines is a deep triangular fossa in which rest the inferior canines in the closed mouth, so that these teeth are covered by the upper lip and probably never lie external to it or become as long as in the hog. Perhaps the most important osteological character is the formation and position of the glenoid cavity for articulation with the lower jaw. It is concave and comparatively very small being only 0·7 of an inch in median transverse diameter, and situated in the middle of the zygomatic arch 1·3 inches in front of the styloid process. In the hog this process is an inch wider, flat, and rests upon the posterior root of the zygomatic arch, and impinges upon the body of the temporal bone, and its posterior border lies on nearly the same transverse plane as the styloid process. Other

peculiarities might be noticed, as the comparatively large external auditory meatus occupying a position much anterior and inferior to that of the hog. The orbit is more nearly closed by bone than in the hog.

These differences seem to indicate an animal more carnivorous than the hog while the comparative smallness of the superior portion of the occiput suggests less of a rooting disposition. I may here suggest, that, could it be shown that in his day, large mollusks abounded in the soft bottoms of shallow streams and estuaries, the conditions of his comfortable existence would seem to have been fulfilled.

A comparison with other living genera of this family shows a very near relationship to the peccaries. Prof. J. S. Newberry assures me that in a comparison with the skeleton of the Mexican and the South American peccary, it is found to be twice as large as the Mexican and a little larger than the South American. No detailed comparison of the entire skeleton has yet been made, but on comparing the heads of the different species there are differences apparently sufficient to constitute *three* distinct species, making the fossil one under discussion a new species. The most striking peculiarities, however, of the head of the fossil species are the small incisors, somewhat larger canines, thinner and more compressed form of head, and the eversion of the lower and posterior angles of the lower jaw.

Portions of this fossil animal have frequently been found, in several states of the Union, but there is no record of anything more than a portion of a jaw, or a cranium, found in any one locality. To find a round dozen of nearly entire skeletons in one locality is therefore most extraordinary, and I hope and trust that out of this quantity of material some comparative anatomist will not only determine its place in the scale of Creation, but will make a precise description of it.

Change by Gradual Modification not the Universal Law.

By Thomas Meehan, of Germantown, Pa.

Natura non facit saltum has been accepted as a grand canon by most naturalists, and the evident absence of connecting links has been thought fatal to theories of evolution. My studies in plant life lead me to the belief that one form will spring from another essentially different, and without any gradual or insensible modifications uniting them.

In any theory of evolution morphology must take an essential part. In the transformation of leaf blade to the various organs of a plant, the change is sometimes gradual, as in the passage from leaves to bracts in some orders, or from sepals to petals, petals to stamens, or stamens to pistils in others; but the cases where the change is from one form of structure to another of very different character are by no means rare, if indeed it is not the general rule; and if it can be proved that change with gradual modification, and change by the sudden appearance of a distinct form, are both good morphological laws in plant life, there is no reason why both laws may not operate in any scheme in which morphology is called to act.

How suddenly the parts of plants often change is well illustrated in most coniferous plants. In *Pinus* on the first pushing of the axis from the cotyledonous condition of the plant, flat leaves are developed often an inch or more in length. After some time these leaves are suddenly arrested, and the axillary buds as suddenly develop into fascicles of needles as they are popularly called. So when the plant reaches its floral condition, the transformation of leaf and stem into the various parts which constitute a cone comes on with wonderful suddenness. The leaf, which we saw so suddenly arrested in early life, now becomes a bract; the fasciculi combine and form the scale, and the axis suddenly ceases to elongate, and gives form to the whole. The one in search of missing links would be sadly puzzled here. This sudden change of folial organs to organs of inflorescence is very common. In the change of one portion of a leaf structure to another Magnolia and Liriodendron afford an interesting example. In many plants the regular leaf blade is metamorphosed and forms the petal; but in these as soon as the plant reaches its flowering stage, the leaf blade is suddenly and entirely arrested

in the formation of the petals, and the stipules are as suddenly developed. The petals are in fact highly developed and transformed stipules, and all without the slightest trace of gradual modification. So in sexual transformations the modifications are by no means always gradual. One who had never seen the maize growing would hardly believe the tassel and the ear were from one plant; yet they are formed morphologically on the same plan, and once in a while we find male flowers gradually merging into females, and females to males, in the respective domain of each, but this is the great exception to the general rule. *Natura non facit saltum* is not true of individual plant life.

Now we come to variations from specific form and here I find great changes with no transitional forms between. I have a *Halesia* from seed of *Halesia tetraptera* which any one might be pardoned for placing in a new genus. It is much farther removed from its parent than *H. diptera* is. The corolla is not drawn up into a funnel-like tube as in the original, but is cupular and barely the length of the stamens. The leaves are broadly ovate, and rugose, and no one at first glance would take it for a *Halesia*. When I first saw it in the seed bed I supposed it to be a young apple tree. I have on my grounds large quantities of *Yucca filamentosa*. Hundreds of plants throw up their flower spikes and open their first blossoms within twenty-four hours of each other. But some years ago one struck off to have a more branching panicle, and to open its flowers *two weeks* before the others, which characters remain and are continued in the progeny. This two weeks was not gained gradually day by day through successive generations, but in one great leap. So with raising peach and other variety of fruit. Though the progeny usually take to the general habits of the parent, there will be once in a while very late varieties from seeds of early ones, and very early from late kinds. In Delaware and New Jersey *Azalea viscosa* varies to a form having glaucous saliciform leaves. I have not raised these up from the seeds, but I have seen an extreme form, with leaves looking rather like the English woodbine (*Lonicera Periclymenum*) than an ordinary *Azalea viscosa*, growing under circumstances which left no doubt on my mind that it sprung without any intermediate links from the other form. *Glyptostrobus* is another remarkable case. I exhibited, at our meeting at Chicago, branches from a tree raised from seed of the *Taxodium distichum*, and branches from an acknowl-

edged *Glyptostrobus pendulus* and no one could separate them. Here is a leap at once to a new genus. Moreover I have a *Thuja* growing which the highest authorities insist is a *Retinospora* but which I know was raised directly from *Thuja occidentalis* without any intermediate parent whatever. Some genera seem very variable—take our common ox-eye daisy (*Chrysanthemum leucanthemum*) for instance; and then, say in *Staphylea*, we may examine hundreds of plants without any apparent variation: but in the variable genera it is a mistake to suppose that these variations are by gradual modifications, though one can often place them so as to appear like gradually approaching links. It is wonderful how many variations there are in the common ox-eye daisy. I have studied them closely for years and find that the most divergent forms are often parent and child. Steady-going species act in the same way. I do not know anything that more exactly repeats itself than *Glycine frutescens*. Every leaflet is exactly ovate with an upward turn from the midrib: but in its variety, known in gardens as *G. magnifica*, the leaves are regularly attenuated and reflexed, and the whole manner so different that some have supposed it a distinct species with the name as above.

Not only do strikingly distinct forms come suddenly into existence, but once born they reproduce themselves from seed, and act in every respect as acknowledged species. The peach in its general form has its branches at a very acute angle upwards. I never saw a plant with branches approaching a right angle, but some thirty years ago one came into existence with a strong angle downwards—a weeping variety, and seeds of this reproduce this form exactly in every respect. So with color. The peach is of a uniform green, no tendency to vary its shade; but ten years ago a deep blood-leaved variety appeared. The deep blood-leaved beech is also a rapid jump from the green, and reproduces the blood-leaved character from seeds. The Siberian arbor vitæ is another great leap from the *Thuja occidentalis*, and reproduces itself from seeds as do all the striking forms in which this species abounds. Indeed I think I may close this branch of my subject by the statement that in over a quarter of a century of experience among living plants, I have rarely known any striking form to have originated by gradual modifications, but always by one great leap. The slight changes are generally in efforts backwards; as when we sow purple beech seed, some few are a trifle

paler than their parents; there is little of this hesitation in the forward leap.

But even reversions are not always gradual. Some years ago the common Babylonian willow sent out branches suddenly which bore singularly curved leaves, just as the nectarine is said to have suddenly sprung from the peach. The cuttings grew and maintained the character. It is known as *Salix Babylonica annularis*. A few years ago I saw a tree, perhaps twenty-five years old, push out the regular weeping willow leaves and I have heard of one in Boston which did the same.

This fact in regard to the willow suggests another great principle. Forms are not only called into existence suddenly, widely different from parents, and can reproduce themselves from seed, but they come into existence without seed agency, and the same or similar form in widely separated localities, and not all necessarily by seed from one individual. I have had sent me from five different localities, two in Pennsylvania, one in New York, one in Illinois and one in Indiana, flowers of *Viola pedata* in which the two upper petals were of the beautiful maroon characteristic of the pansy. Then I have given what I think good reasons, in Proc. Ac. Nat. Sciences, for believing that identical forms of blackberries and raspberries originate in distinct localities. Again a whole change of character will occur suddenly in many individuals through a large extent of country. This season, in our part of the world, at least half the leaves of the Liriodendron are from five to seven-lobed, when, as is well known, the three-lobed character has been almost specific in former years. We are accustomed to say about these changes that they are "caused by climate," but this expression proves nothing. We have in Pennsylvania a form of *Viola cucullata* usually growing in wet places, which always causes the breast of the young botanist to thrill with the idea that he has a new species. The paler color and more delicate growth when the plants are seen in the aggregate are very striking. But when the plants and flowers are analyzed, no difference is found that can be described in words. Those to whom reason comes rapidly tell us at once that the difference is "caused" by the plant growing in wet places; but we soon find the deep purple form coming down as it were from the high ground to the swamp and the pale one working up to the high ground, yet each retaining its little peculiarity. There is a

gradual modification of location going on, a gradual modification of circumstances, but no gradual modification of form to correspond.

In suggesting that species come suddenly into existence, not always from one parent, and not only in one locality, I wish to confine myself mainly to facts, and to facts within my own observation; but I cannot forbear suggesting how much nearer such a theory coincides with possibilities, than the theory of descent by gradual modifications from a single parent; where, surrounded by innumerable foes, not even the "fittest" could survive. As a host of Liliputians could finally subdue a single Brobdignagian, so the sheer crowd of individuals in an old form in the struggle for life would crowd out the solitary new one, no matter how much better fitted it might be, *per se*, to survive. Insect agency alone, as I have shown in a paper on *Linaria vulgaris* in Proc. Ac. Nat. Sciences, and I believe Dr. Asa Gray before me in Silliman's Journal, is a powerful force in the crushing out of any departure from a parent form.

Again a theory deserves consideration when it will explain phenomena in which others fail. In 1862 I discovered the identity of relative characters in allied species of European and American trees. Mr. Darwin frankly admits in his work on "Animals and Plants under Domestication," that the facts given in my paper are unexplainable by his theory of natural selection; and St. George Mivart makes strong use of them against Darwin in his "Genesis of Species." That the variations of allied plants should be all on one plan is not consistent with any idea that the selection of their forms should be for the purpose of a struggle with one another, if they had any such power of selection. The American hornbeam (*Carpinus Americana*) differs from the European (*C. Betulus*) amongst other points, in being less sharply and deeply serrate. But on examining hornbeam leaves it is seen that though simple, they are on the road to pinnation. The lateral veins are typical midribs of pinnules. In the American there are from five to ten small serratures between each main one that terminates the main rib, while in the European there are but from two to five. The American has more nearly approached a pinnate form, and bearing the case of the Liriodendron in mind we see how this can be brought about without the necessity of going through any gradual modification from a single parent, but in obedience probably to

some law of nutrition regulating form which may operate at times on a large number of individuals, and at others on a single cell. That form is in some way dependent on nutrition is demonstrated not only by my papers before this Association, on the laws of sex, but by the operation of various fungi, which as they attack plants, each in its own way, produce fixed abnormal forms in these plants, each after its own kind. The whole habit of a plant will sometimes change under these attacks. In the Rocky Mountains I have seen a Euphorbia, probably *E. humistrata*, or a form of *E. maculata*, usually creeping, become wholly erect, when an *Æcidium* was growing on it. The attack of course interferes with nutrition and we may therefore say that in some cases nutrition governs form. Sudden cosmical disturbances, acting on nutrition, would therefore constitute a factor equal to great specific changes of form, and we are not compelled to look solely to gradual modifications of form from single individuals for the explanation of all the phenomena we see.

A review of the facts I have presented shows, I believe, the following truths: —

1. Morphological changes in individual plants are by no means by gradual modifications.

2. Variations from specific forms follow the same law.

3. Variations are often sudden and also of such decided character as to be deemed generic.

4. These sudden formations perpetuate themselves, and act in all respects the same, as forms which spring through gradual modifications.

5. Variations of similar character occur in widely separated localities.

6. Variations occur in communities of plants simultaneously by causes affecting nutrition and perhaps by other causes.

Arguing from these, new and widely distinct species may be suddenly evolved from preëxisting forms without the intervention of connecting links.

On the Cotton Worm of the Southern States (*Aletia argillacea* Hübner). By Aug. R. Grote.

The earliest[1] scientific name for the cotton worm is given by Jacob Hübner in the second hundred of his "Supplement to the Collection of Exotic Lepidoptera," dated 1822. The moth is there figured in two positions under the numbers 399 and 400, and described under the name *Aletia argillacea* on page 32. Although the insect has subsequently received different names, this name of Hübner's is the one it should in future bear. For the name "*Anomis xylina*," now in scientific use, I am responsible. In the year 1864, in the Proceedings of the Entomological Society of Philadelphia, I referred the *Noctua xylina* of Thomas Say to Hübner's genus Anomis, as defined by M. Guenée, and regarded as synonymous the *Anomis bipunctina* of the latter author.

With the true type of the genus Anomis, the *Anomis erosa* of Hübner, I have since then become familiar, and I find that it differs structurally and generically from the cotton worm moth, which latter must accordingly remain under the combined title originally proposed for it by Hübner.

The different stages of Aletia, as it is found throughout the cotton belt of the Southern States, have been faithfully portrayed by Professor Townend Glover, of the Agricultural Department in Washington. On the Professor's plates numerous other insect depredators on the cotton plant are excellently portrayed, and this work (I believe as yet unpublished) ought certainly to be issued by the Legislatures of the different states interested in cotton culture, or indeed by the General Government, and publicly distributed, so that a knowledge of the economy of these parasites be diffused. For his manuscript work, Professor Glover has indeed received a medal from the late Emperor of the French (a nation fortunately profuse in acknowledgment) but, if I am correctly informed, no more substantial reward has as yet crowned Professor Glover's praiseworthy efforts for the advancement of knowledge and the consequent amelioration of his race.

In the Second Report on the Insects of Missouri, Professor C. V. Riley notices the cotton worm, and illustrates the moth by a

[1] I am indebted to Dr. Hagen for the bibliography of the *Noctua gossypii* of Fabricius. I believe this to be a distinct species from the *Aletia* and probably belonging to a different family.

woodcut, in which the insect is represented head downward in a state of rest. The moth is drawn in this position on the authority of a gentleman in Texas, and the subject is treated throughout, and indeed necessarily, by Professor Riley, at second-hand. In Professor Riley's Sixth Report (published this year) the cotton worm is again discussed under similar conditions, while the position of the moth in a state of rest has now become normal. It is however claimed, in this Report, that the cotton worm "hibernates" as a moth, and the credit of this observation is given to the Second Report, while the discovery of the fact is claimed to have been made by what Professor Riley calls the process of "analogy."

It is the object of the present paper to throw, happily, some light on the biography of the cotton worm as it occurs in the Southern States, and in so doing I think it will become apparent that Professor C. V. Riley has regarded the same subject from an erroneous standpoint, having considered the cotton worm as belonging to our fauna, and accordingly misunderstood its economy as displayed with us and far from its natural abode. And here, while I am obliged to differ on a scientific question with Prof. Riley, I bear willing testimony to the great good achieved by the publication of the Missouri Reports.

The *Aletia argillacea*, or cotton worm, is an insect belonging to the Noctuæ, a group of nocturnal moths. It is one of a number of intertropical or southern forms, somewhat nearly allied to our more thickly scaled and northern genus Plusia. The caterpillar is a "half-looper," to use a common term, and the chrysalis is held within an exceedingly loose web on the plant, the few threads usually binding over the edge of the leaf and of themselves furnishing no adequate protection to the pupa. [I here exhibit to the Association specimens of the larva, pupa and moth of Aletia]. Technical descriptions of the different stages are already extant and so may be passed over here. The more immediate question for our solution is the consecutive history of the insect, so that we may be prepared to offer suggestions to the agriculturists for its destruction.

The region over which, during five seasons, I have observed the cotton worm, embraces the central portion of the cotton belt in the states of Georgia and Alabama, and in particular the counties of Marengo and Greene, lying along the Tombigbee and Black

Warrior rivers. There cotton is planted in March and April, blooms in June and July, and perishes in November or with the frost. The earliest period at which I have noticed the young worm was the last week in June, and its usual appearance was in July, sometimes as late as the latter part of the month. Its date of appearance was irregular, and never accurately coincided in any two seasons. Sometimes it seemed as though we were "*not going to have any worm at all this year*," a remark suggested by hope and the tardiness of its advent. My observations have been mainly directed to the question of the origination of the first brood and have led me to record the following results. I have observed that the appearance of the worm in the fields was always heralded by flights of the moth, which came to light in houses at least a week before the worm was noticed on the plants. I have observed that the distribution of the first brood was irregular; the worms occurring here and there over miles of country, while infesting some plantations, skipping unaccountably others which the second brood, however, seldom failed to reach. I have noted that the worm was always heard of to the southward at first, and never to the northward, of any given locality in the cotton belt. Finally, after diligent search, no traces of the insect in any stage could be found by me during the months *preceding* the appearance of the first brood heralded by the moth, and *after* the cotton was above the ground. The broods themselves were consecutive and without interruption so long as the conditions were favorable. The last brood, in years where the worm was numerous, eat up every portion of the plant that was at all soft, flowers, the persistent calyx, the very young boll, the terminal shoots. The last brood of worms changed into chrysalides in myriads on the leafless stems, clinging by their few threads as best they might, and disclosed the moth in the face of the frost, many of the chrysalides perishing. Afterwards, on sunny winter days, I have noticed the live moth about gin houses and fodder stacks, or the negro quarters. Was this a true "hibernation" or merely an accidental survival? The locality and the condition seem to me alike artificial.

Now Hübner describes the moth of the cotton worm at first, as from Bahia. Sufficient testimony to the identity of our insect with one destructive to the West Indian, Mexican and Brazilian perennial cotton, is at hand and the fact is established. In a classificatory point of view, the affinities of the cotton worm are

with southern rather than northern forms of its family, as I have already pointed out. The conclusion to which I have come with regard to the cotton worm is, *that it dies out every year (with its food plant) that it occurs in the cotton belt of the Southern States, and that its next appearance is the result of immigration.* Testimony is at hand to show that for many years after the cultivation of the cotton plant was introduced into the Southern States, the cotton worm never appeared. The date at which it first appeared in Central Alabama has been differently stated to me, but it evidently but little preceded the late war. That the moth is capable of sustaining long and extended flight is readily proven. Professor Packard observed the moth off the coast of the Eastern States, as also Mr. Burgess. I have observed the moth in October in Buffalo, N. Y., as also Dr. Harvey. According to Mr. Riley the moth has been observed in Chicago, I presume in the Fall. It seems that the moth follows the coast-line northward as also the water courses that empty into the Gulf of Mexico. It is noteworthy here that the water-shed of the Ohio and Mississippi, extends to within fifty miles of Buffalo. As an example of the prolonged flight of moths, I will state, that I have observed in the Gulf Stream, off the Carolinas and out of sight of land, in the month of August, large numbers of a moth, the *Agrotis annexa* of Treitschke.

Again I have been struck by the absence of parasitic checks to the cotton worm in the south. I could never discover any, although such may exist. Spreading as I believe it to do, as a moth, the absence of peculiar parasites to the worm may be reasonably accounted for. I have already and elsewhere pointed out, that in order to make the first brood of the cotton worm the progeny of the so-called "hibernating" individuals (as Professor Riley would suppose), a period of several months has to be accounted for, since these "hibernating" moths could not wait till midsummer to deposit their eggs; and while the cotton is young, and even before it is up, insect life is active, and the weather is warm and other vegetation fully out in the region of the South where I have lived. There is also no reason to believe that the cotton worm ever breeds in the North, and this, notwithstanding Professor Riley's suggestions to the contrary, in the Sixth Report before mentioned. The worm never has been noticed on any other plant than the cotton, and in the south perishes by thou-

sands rather than eat any other. The habit of wandering in masses when food fails is a proof of this, as while the worm is supplied with cotton leaf it never quits the plant, transforming to the chrysalis on the stalk which has furnished it nutriment. The wandering habit is not normal but accidental, and the worm is not "gregarious" like the "tent caterpillar." Its "hibernation" with us must also be regarded as accidental, or at least as barren of results. For when spring comes the *Aletia argillacea* has vanished, and is not found with the hibernating species of Lepidoptera, renewedly active. And if it *were* found in February and March, it would find no cotton plants upon which to deposit its eggs. If oviposition ever takes place in these months in the cotton belt, the young cotton, free from worms, disproves its efficacy.

It is possible that in the southern portions of Texas, or the Floridian peninsula, the Aletia may sustain itself during the entire year; I have no means of information on this point. My observations are made on its occurrence over the central and principal portions of the cotton belt and into which I believe it to be imported *de novo* every season that it there occurs and from more southern regions.

I conclude, therefore, that while the cotton plant is not indigenous to the Southern States (where it becomes an annual), the cotton worm moth may be considered not a denizen, but a visitant, brought by various causes to breed in a strange region, and that it naturally dies out with us in the cotton belt, unable to suit itself *as yet* to the altered economy of its food plant and to contend with the changes of our seasons.

When this fact is comprehended, it will simplify the process of artificial extermination by limiting the period during which we can successfully attack the cotton worm, and by doing away with a certain class of proposed remedies.

From the foregoing it will be evident that 1. The artificial agent employed to destroy the cotton worm must be employed against the first brood as it appears in any given locality during the progression of the moth northward; and 2. That, in order to be effectual, a concerted action in the application of the remedial agent in any given locality will be found necessary.

I also recommend the introduction of the English sparrow into the Southern States, and additional legal protection to insectivorous birds. Since the war there has been too much ignorant

use of the gun on the part of the negroes. *All* the birds should be protected as much as possible, for many species not usually considered insectivorous are yet found, during certain seasons of the year, to live on insects.

I offer the following as the synonymy of the cotton worm in scientific literature:

Aletia argillacea Hübner, Zutr. 3d Hund., S. 32, figs. 399–400 (1822).

Noctua xylina Say,[2] Sec. Ed. Vol. 1, p. 370 (1859).

Anomis grandipuncta Guenée, Noct., Vol. 2, p. 400 (1852).

Anomis bipunctina Guenée, Noct., Vol. 2, p. 401; id. Vol. 3, p. 397 (1852).

Anomis xylina Grote, Proc. Ent. Soc. Phil., Vol. 3, p. 541 (1864).

Anomis xylina Riley, 2nd Mo. Rep. p. 40, fig. 13 (1870).

Anomis xylina Grote, Rural Carolinian, 3, p. 88 (1871).

Anomis xylina Riley, 6th Mo. Rep. p. 17 (1874).

Aletia argillacea Grote, List of the Noctuidæ of N. America, p. 24.

ON THE INSECTS MORE PARTICULARLY ASSOCIATED WITH SARRACENIA VARIOLARIS (SPOTTED TRUMPET-LEAF). By CHAS. V. RILEY, of St. Louis, Mo.

THE insect-catching powers of those curious plants, the Fly-traps (*Dionæa*), the Sun-dews (*Drosera*), and the Trumpet-leaves (*Sarracenia*), have always attracted the attention of the curious, but renewed interest has been awakened in them by virtue of the interesting experiments and observations on their structure, habit and function, that have lately been recorded, and especially by the summing up of these observations in some charming papers by Prof. Asa Gray, which recently appeared in "The Nation" and the "New-York Tribune" under the title of "Insectivorous Plants."

Through the courtesy of Dr. J. H. Mellichamp of Bluffton, and of H. W. Ravenel of Aiken, South Carolina, who have sent me abundant material, I am able to submit the following notes of an entomological bearing, on the Spotted trumpet-leaf (*Sarracenia va-*

[2] In a letter to C. W. Capers, dated Nov. 18th, 1827. I do not know whether this letter is elsewhere published, but this question will not affect the synonymy here proposed.

riolaris), which must henceforth rank with the plants of the other genera mentioned as a consummate insect-catcher and devourer.

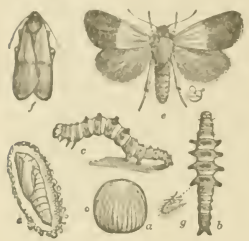
The leaf of Sarracenia is, briefly, a trumpet-shaped tube, with an arched lid, covering, more or less completely, the mouth. The inner surface, from the mouth to about midway down the funnel, is covered with a compact, decurved pubescence which is perfectly smooth and velvety to the touch, especially as the finger passes downward. From midway it is beset with retrorse bristles, which gradually increase in size till within a short distance of the bottom where they suddenly cease, and the surface is smooth. There are also similar bristles under the lid. Running up the front or ventral side of the trumpet is a broad wing with a hardened border, parting at the top and extending around the rim. Along this border, as Dr. Mellichamp discovered, but especially for a short distance inside the mouth, and less conspicuously inside the lid, there exude drops of a sweetened, viscid fluid, which, as the leaf matures, is replaced by a white, papery, tasteless, or but slightly sweetened sediment or efflorescence; while at the smooth bottom of the pitcher is secreted a limpid fluid possessing toxic or inebriating qualities.

The insects which meet their death in this fluid are numerous and of all orders. Ants are the principal victims, and the acidulous properties which their decomposing bodies give to the liquid doubtless render it all the more potent as a solvent. Scarcely any other Hymenoptera are found in the rotting mass, and it is an interesting fact that Dr. Mellichamp never found the little nectar-loving bee or other Mellifera about the plants. On one occasion only have I found in the pitcher the recognizable remains of a Bombus, and on one occasion only has he found the honey-bee captured. Species belonging to all the other orders are captured, and among the larger species that I have most commonly met with, which, from the toughness of their chitinous integument, resist disorganization and remain recognizable, may be mentioned *Asaphes memnonius* and *Euryomia melancholica* among Coleoptera, *Pentatoma lugens* and *Orsilochus variabilis*, var. *complicatus* among Heteroptera: while katydids, locusts, crickets, cockroaches, flies, moths, and even butterflies, and some Arachnida and Myriopoda, in a more or less irrecognizable condition, frequently help to swell the unsavory mass.

But while these insects are decoyed and macerated, in order, as

we may naturally infer, to help support the destroyer, there are, nevertheless, two species which are proof against its siren influences and which, in turn, oblige it either directly or indirectly to support them.

The first is *Xanthoptera semicrocea* Guen., a little glossy moth, which may be popularly called the Sarracenia moth. It is strikingly marked with gray-black and straw-yellow, the colors being sharply separated across the shoulders and the middle of the front wings. This little moth walks with perfect impunity over the inner surface of the pitcher, which proves so treacherous to so many other insects. It is frequently found in pairs within the pitchers soon after these open, in the early part of the season or about the end of April. The female lays her eggs singly, near the mouth of the pitcher, and the young larva, from the moment of hatching, spins for itself a carpet of silk and very soon closes up the mouth by drawing the rims together and covering them with a delicate, gossamer-like web, which effectually debars all small outside intruders. It then frets the leaf within, commencing under the hood and feeding downward on the cellular tissue, leaving only the epidermis. As it proceeds, the lower part of the pitcher above the putrescent insect collection becomes packed with ochreous excrementitious droppings, and by the time the worm has attained its full size the pitcher above these droppings generally collapses. This worm when fully grown is beautifully banded transversely with white and purple or lake-red, which Dr. Mellichamp poetically likens in brightness to the Tyrian dye. It is furthermore characterized by rows of tubercles, which are especially prominent on the four larger legless joints. It is a half looper, having but six prolegs, and keeps up, in travelling, a constant restless, wavering motion of the head and thoracic joints, recalling *paralysis agitans*. The chrysalis is formed in a very slight cocoon, usually just above or within the packed excrement. The species, kindly determined by Mr. A. R. Grote, was many years ago figured by Abbot, who found it feeding on *Sarracenia variolaris*.



XANTHOPTERA SEMICROCEA. — *a* egg, enlarged, the natural size indicated at side; *b*, *c*, larva, back and side views; *d*, chrysalis; *e*, moth, normal form, with wings expanded; *f*, pale variety, with wings closed.

in Georgia. Guenée's descriptions were made from these figures, for which reason I have made some descriptive notes from the living material.[1]

The species feeds alike on *S. variolaris* and *S. flava*, and there are at least two broods each year, the first brood of larvæ being found during the early part of May, the second toward the end of June, and disappearing with the dying of the leaves.

The second species is a still more invariable living accompaniment of both kinds of Sarracenia mentioned. By the time the whitish efflorescence shows around the mouth of the pitcher, the moist and macerated insect remains at the bottom will be found to contain almost invariably a single whitish, legless grub or "gentile," about as large round as a goosequill, tapering to the retractile head, which is furnished with two curved, black, sharp hooks; truncated and concave at the posterior end of the body.

This worm riots in the putrid insect remains, and when fed upon them to repletion bores through the leaf just above the petiole and burrows into the ground. Here it contracts to the pupa state, and in a few days issues as a large two-winged fly, which I have described (*loc. cit.*) as *Sarcophaga sarraceniæ* — the Sarracenia flesh-fly.

SARCOPHAGA SARRACENIÆ.— *a*, larva, *b*, pupa; *c*, fly, the hair lines showing average natural lengths; *d*, enlarged head and first joint of larva, showing curved hooks, lower lip (*g*), and prothoracic spiracles; *e*, end of body of same, showing stigmata (*f*) and prolegs and vent; *h*, tarsal claws of fly with protecting pads; *i*, antenna of same. All enlarged.

The immense prolificacy of the flesh-flies, and the fact that the young are hatched in the ovaries of the parent before they are deposited by her on tainted meat and other decomposing or strong-smelling substances, have long been known to entomologists, as has also the rapid development of the species. The viviparous habit among the Muscidæ is far more common than is generally supposed, and I have even known it to occur with the common house-fly, which normally lays eggs. It is also possessed by some Œstridæ, as I have shown in treating of *Œstrus ovis*, the sheep bot-fly.[2]

[1] These will be found in the Transactions of the St. Louis Academy of Science.
[2] 1st Mo. Ent. Rep. p. 165.

But the propensity of the larvæ for killing one another, and their ability to adapt themselves to different conditions of food-supply are not sufficiently appreciated. I have long since known, from extensive rearing of parasitic Tachinidæ, that when, as is often the case, a half dozen or more eggs are fastened to some caterpillar victim only large enough to nourish one to maturity, they all hatch and commence upon their common prey, but that the weaker eventually succumb to the strongest and oldest one, which finds the juices of his less fortunate brethren as much to his taste as those of the victimized caterpillar. Or, again, that where the food-supply is limited in quantity, as it often is and must be with insects whose larvæ are parasitic or sarcophagous, such larvæ have a far greater power of adapting themselves to the conditions in which they find themselves placed, than have herbivorous species under like circumstances.

Both these characteristics are strongly illustrated in *Sarcophaga sarraceniæ*. Several larvæ, and often upward of a dozen, are generally dropped by the parent fly, within the pitcher; yet a fratricidal warfare is waged until usually but one matures, even where there appears macerated food enough for several. And if the Xanthoptera larva closes up the mouth of the pitcher ere a sufficient supply of insects has been captured to nourish it properly, this Sarcophaga larva will nevetheless undergo its transformations though it sometimes has not strength enough to bore its way out, and the diminutive fly escapes from the puparium, only to find itself a prisoner unless deliverance comes in the rupture or perforation of the pitcher by the moth larva or by other means. This rupturing of the pitcher does not unfrequently take place, for Dr. Mellichamp writes under date of June 27, as follows: "Most old leaves now examined—I might almost say all—instead of being bored, seem ripped or torn, as if by violence, apparently from without. You see occasionally shreds of the leaf hanging. Surely the legless larva of Sarcophaga cannot do this! What then—toads, or frogs, or crawfish abounding in these moist pine lands? or rather is not the fat maggot the occasion of the visits of the quail which lately I have observed here?"

These two insects are the only species of any size that can invade the death-dealing trap with impunity while the leaf is in full vigor, and the only other species which seems at home in the leaf is a minute pale mite belonging, apparently, to *Holothyrus* in the

Gamasidæ, and which may quite commonly be found crawling within the pitcher; and a small Lepidopterous leaf-miner, which I have not succeeded in rearing. There must, however, be a fifth species, which effectually braves the dangers of the bottom of the pit, for the pupa of Sarcophaga is sometimes crowded with a little Chalcid parasite, the parent of which must have sought her victim while it was rioting there, as larva.

No other insects, so far as we now know, can crawl up the slippery belt, but tumble into the tube and there meet their death.

Certain questions very naturally present themselves here: First, What gives the flesh-fly more secure foothold on the slippery pubescence than the common house-fly exhibits? Second, What enables the larva of the flesh-fly to withstand the solvent property of the fluid which destroys so many other insects? Third, What gives the Sarracenia moth and its larva security? I can only offer in answer, the following suggestions: The last joint of the tarsus of the common house-fly has two movable, sharp-pointed claws and a pair of pads or "pulvilli." These pads were formerly supposed to operate as suckers, and all sorts of sensational accounts of this wonderful sucker have been given by popular writers, who forgot that there are any number of minute insects having no such tarsal apparatus, which are equally indifferent to the laws of gravitation so far as walking on smooth, upright surfaces, or on the ceiling, is concerned. In reality, these pads are thickly beset on the lower surface with short hairs, most of which terminate in a minute expansion kept continually moist by an exuding fluid—a sort of perspiration. Take the soft human hand, moistened by perspiration or other means, and draw it, with slight pressure, first over a piece of glass or other highly-polished surface, and then over something that has a rougher surface, such as a planed board, a papered wall, or a velvety fabric, and you will experience much greater adhesion to the smoother objects, and may understand the important part which these moist pads play in the locomotion of the fly. They also act, in part, like the cushions of a cat's paw in protecting and preventing abrasion of the claws, which are very useful on the rougher surfaces, where the pads are less serviceable.

Now, compared with *Musca domestica*, the claws of *Sarcophaga sarraceniæ* are much the longer and stronger, and the pads much the larger, presenting three or four times the surface. These dif-

erences are, I think, sufficient to explain the fact that while the common fly walks with slippery and unsteady gait on the smooth pubescence (the retrorse nature of this pubescence sufficiently explaining the downward tendency of the movement), its sarcophagous congener manages to get a more secure footing; for not only does the latter present a larger adhesive surface, but the longer claws are more likely to reach beyond the pubescence and the bristles, and fasten to the cellular tissue of the leaf beyond. Moreover Sarcophaga is more thickly beset with stiff, spinous bristles than Musca, and Dr. Mellichamp says that when disturbed it buzzes violently about, just as if an animated sheep-bur had fallen into the tube—not apt to go down, because it will hitch and stick, and finally, by main force, it generally emerges, but once in a while also succumbs.

In answer to the second question, I can only say that there is nothing exceptional in the power of the larva to withstand the solvent quality of the fluid; it is, on the contrary, in accordance with the facts known of many species of Muscidæ and Œstridæ, some of which, like the well known horse-bot, revel in a bath of chyme, while others are at ease in the intestial heat of other warm-blooded animals. It is also well known that they will often live for hours in strong liquids, such as alcohol and turpentine.

In answer to the third question, the moth is doubtless assisted in walking within the tube by the spines and spurs on the legs which it, in common with most other moths, possesses; the tarsi in Xanthoptera being armed with spines, and the spurs being quite long, and in *semicrocea* usually shod at tip with a corneous point. Its larva overcomes the treacherous surface by either carpeting it with silk, or destroying it.

To one accustomed to seek the why and wherefore of things, the inquiry very naturally arises as to whether Xanthoptera and Sarcophaga play any necessary or important rôle in the economy of Sarracenia. Speaking of the Sarcophaga larvæ, Mr. Ravenel asks, "May he not do some service to Sarracenia as Pronuba does to Yucca?" And if so may not all this structure for the destruction of insects be primarily for his benefit? Can he be merely an intruder, sharing the store of provision which the plant, by ingenious contrivance, has secured for itself, or is he a welcome inmate and profitable tenant? Self-fertilization does not take place in Sarracenia, and the possibility that the bristly

flesh-fly aids in the important act of pollination, lends interest to the facts. No one has witnessed with greater pleasure than myself the impulse which Darwin has of late years given to such inquiries; but we should be cautious lest the speculative spirit impair our judgments, or our ability to read the simple lesson of the facts. My own conclusions summed up are:

First: There is no reason to doubt, but every reason to believe, since the observations of Dr. Mellichamp, that Sarracenia is a truly insectivorous plant, and that by its secretions and structure it is eminently fitted to capture its prey.

Second: That those insects most easily digested (if I may use the term) and most useful to the plant are principally ants and small flies, which are lured to their graves by the honeyed path, and that most of the larger insects, which are not attracted by sweets, get in by accident and fall victims to the peculiar mechanical structure of the leaf.

Third: That the only benefit to the plant is from the liquid manure resulting from the putrescent captured insects.

[Mr. Ravenel, in making a transverse section near the base of the young leaf, noticed large tubular cells passing down through the petiole into the root, and much of the liquid manure may possibly pass through these into the root stalk].

Fourth: That Sarcophaga is a mere intruder, the larva sponging on and sharing the food obtained by the plant, and the fly attracted thither by the strong odor, as it is to all putrescent animal matter or to other plants, like *Stapelia variegata*, which give forth a similar odor. There is nothing to prove that it has anything to do with pollination, and the only insect that Dr. Mellichamp has observed about the flowers with any frequency, is a Cetoniid beetle—the *Euryomia melancholica*—which, with other species of its genus, is commonly found on many different flowers.

Fifth: That Xanthoptera has no other connection with the plant than that of a destroyer, though its greatest injury is done after the leaf has performed its most important functions. Almost every plant has its peculiar insect enemy, and Sarracenia, with all its dangers to insect life generally, is no exception to the rule.

Sixth: That neither the moth nor the fly has any structure peculiar to them that enables them to brave the dangers of the plant, beyond what many other allied species possess.

NATURAL EROSION BY SAND IN THE WESTERN TERRITORIES.[1] By G. K. GILBERT, of Rochester, N. Y.

ABSTRACT.

SAND is efficient in erosion (1) when moved by water, and (2) when moved by air; and in our western territory are to be seen some peculiarly forcible illustrations of both phases of its action.

1. The function of flowing water in denudation is threefold. It dissolves rock; it carries the sand which cuts rock; and it transports the *débris*. Abrasion by clear water is almost *nil;* where it crosses a partly soluble rock, such as an impure limestone, it will half dissolve it and leave on the surface a coat of "rotten stone," softer than the main mass. But where mud or sand is carried by the water, a friction arises, which removes rocks faster than solution can proceed, and all traces of decay are obliterated. In the gorges of the Colorado, where the water is never free from mud, and where a great volume of sand is transported at flood time, the efficiency of sand as a tool of erosion is most conspicuously displayed; not only the rock which *in situ* forms the river channel, but the bowlders, which, brought in, by transient torrents, through side gorges, choke the channel, and cover a large portion of the bed rock, are subjected to the gnawing of the sand. Held by the water, as the dust of an emery wheel is held by its matrix of glue, the sand and mud file away the hardest rock by their incessant friction, and leave on its surface a polish not excelled by art. The whole river bed, as can be seen at low water, is beautifully carved, each texture of rock being given a peculiar pattern of sculpture, but none escaping. Of the rate at which the river eats its bed no measure has yet been found, but no one can scrutinize the bottom, scored all over by its fresh tooth-marks, without being profoundly impressed with the magnitude of the excavation accomplished by the devouring sand.

2. Precisely analogous is the action of dry sand carried by the wind. Upon our deserts, where there is no moisture nor humus to hold together, and no vegetation to entangle sand, it is at the mercy of the wind. By its multitudinous impact and friction, it

[1] The material upon which this paper is based was gathered by the author while in the employ of the U. S. Engineers, and under the direction of Lieut. Geo. M. Wheeler. It will appear officially, and more fully, in the geological volume of the report of Engineer "Explorations and Surveys west of the 100th meridian."

triturates and erodes all substances that come in its way, reducing them and its own particles to a fine dust, that drifts lightly here and there, until it is caught at last by some transient or permanent stream, and finds the bottom of a lake or ocean. Of this attrition the most conspicuous traces are seen on the rocks of mountain passes, and on the pebbles of open plains. The carving of rocks in passes, where the wind is focussed, by the convergence of slopes, to exceptional intensity, is a frequent phenomenon, and has been recorded by many observers. The discovery of the sand blast has recently directed attention to it as a natural operation of the same character, and I have nothing additional to say of it save to note that it is not a mere illustration of the power of wind and sand, but the index of a denuding agent worthy to be mentioned in the list with frost, and flood, and wave. The specimens and the photographs that I am able to submit to you will show, better than any words can describe, the resemblance, and the difference of the sculptures wrought by river and by wind. The water currents have, at any one spot, but a single direction, and the furrows and mouldings of the carved surface are grouped in a single system; but the wind may blow in many directions and produce a scroll of corresponding complexity. In the locality from which I draw my illustrations (Fortification Rock at the head of the Black Cañon of the Colorado) the topography permitted but two winds, and these chiselled the opposite faces of the rock into independent scrolls.

Another phase of the work of the natural sand-blast is less familiar. So far as I am aware, it has been noted only by Dr. J. S. Newberry, who makes brief mention of it in the geology of the Ives Expedition, and by Lieut. Geo. M. Wheeler, who gathered specimens of its results near the mouth of the Virgin River in 1869. At the locality near the Virgin, a smooth and gently sloping plain is strewn with carved pebbles of various rocks. The quartzites and flints are smooth, and shine with a polish that attrition by rolling never gives. The basalts and trachytes have uneven surfaces, with the harder crystals in the prominences. All combinations of hard and soft, as of chert and limestone, exhibit the harder bands in bold relief; while homogeneous and soft pebbles, and especially those of limestone, are carved superficially into vermicular ridges and grooves of the most exquisite arabesque patterns. All this is the work of the drifting sand, swept over the plain by the wind. Below the surface, the plain is built

of a gravel packed in sand, the whole being detritus from neighboring mountains. Only the pebbles at the surface exhibit the carving; all below are merely rounded by rolling and jostling in the currents that originally brought them. The sand blast that carves the surface pebbles, ultimately destroys them completely, and their whole material is swept away to the adjacent rivers. As one layer disappears from the surface, another is brought to view to be similarly treated; and in this way the plain is degraded. What is true of this spot is true of every desert plain—not that everywhere pebbles are visibly carved, but that wherever sand moves freely there results a trituration and comminution that is truly a work of erosion. The degradation of plains by the wind cannot be measured, because it leaves no such monuments as does denudation by water. Water is a leveller in the sense that it transfers material from higher places to lower; but, where it erodes, it always works most rapidly along the lowest lines, and leaves ridges and islands, by which its results can be measured. The wind, on the contrary, works most diligently upon salients, and strives to smooth away every vestige of the surface it remodels.

Yet another phase of erosion by dry sand is to be seen near the Colorado River not far below the accession of Paria Creek. In that region the strata are nearly horizontal, and every great alternation of hard and soft strata is parent to a line of cliffs, the soft stratum yielding more rapidly to erosive agents, and, by its waste, steadily undermining the harder, which succumbs by the fall of blocks to the foot of the cliff. In this particular locality the hard bed is a Triassic conglomerate, and the underlying soft bed, a gypsiferous, Triassic shale. As a rule the agents which disintegrate and remove the shale are frost and rain, but here another agent participates, and even preponderates. The wind sweeps with exceptional force along the base of the cliff, and carrying away with it sand derived chiefly from the disintegration of the conglomerate, not only carves out the shale, but files away every hard block that falls from the top of the escarpment, and keeps the base clean swept. When a bowlder falls on the smooth slope of shale the driven sand seems to be given greater friction around its base, and the shale is hollowed out about it. At the same time, its lower part is cut more rapidly than its upper, and it becomes, in time, a pear-shaped mass, standing with its smaller part downward, and perched on a pedestal of shale *in situ*. Finally it is

completely undermined, and topples over,—only to suffer new attrition upon a new face.

In these several ways, dry sand, wind-borne, contributes to the interminable task of paring away the continent. It undermines cliffs; it scours mountain passes, and it reduces open plains. It is an unobtrusive agent, working as silently as frost, and in desert regions only can it compete with frost in the magnitude of its results. But its results are nevertheless important, and it is deserving of recognition whenever are enumerated the great erosive agents of the earth.

The Recency of Certain Volcanoes of the Western United States. By G. K. Gilbert, of Rochester, N. Y.

ABSTRACT.

Whatever of new material is brought forward in this paper was acquired while I was employed as geologist of exploring expeditions conducted by the U. S. Engineer Corps, and is here presented, with permission, in advance of the official report.

It is hard for one who has not been there, to appreciate the universality of the distribution of lavas in our western mountain region,—the region, that is, which lies between "the Plains" and the Pacific. The country is made up of a multitude of mountain ranges, and each range, with few, or perhaps no exceptions, contains volcanic rocks. Sometimes there are only dikes to be seen, but more frequently conspicuous masses; and often the lavas exclude or cover all other rocks. The principal masses belong to the acidic or trachytic group of lavas, and extensive subsequent denudation shows, in every instance, the antiquity of these; but eruptions of lavas of the basic or basaltic group have been exceedingly numerous, and, as a rule, have occurred later than those of trachyte. To this latter class belong all of the most recent eruptions, and of these I wish especially to speak.

1. The only proof, by superposition, of the recency of western basalts, with which I am acquainted, is found near Fillmore, Utah,

where *coulées* and cones rest upon certain fresh-water beds that I have elsewhere shown reason to refer to the glacial epoch. [Bulletin Phil. Soc. of Washington.] The same beds, in the same neighborhood, overlie other and older flows of the same lava, showing that the epoch of basalt (using the phrase only in a local sense) included the glacial epoch. And, in passing, it may be noted, for the benefit of those who base theories on the littoral distribution of volcanoes, that this locality is six hundred miles from the Pacific ocean.

2. Another proof of the recency of western volcanic action lies in the existence of geysers. Geysers are springs combining a very high temperature with a small discharge, and the only adequate explanation ever offered for their heat is the presence near the surface of a lava mass still hot, and hence freshly intruded.

3. Scattered through the western mountains are basaltic cones and lava beds, upon which decay has formed no soil, and vegetation found no foothold. Save that the iron of the lightest scoria has reddened by the acquisition of oxygen, they lie as black and rugged as the latest eruptions of Vesuvius. These have been described by a number of travellers, and I have myself seen them in Owen's Valley, California, near San Francisco mountain, Arizona, near Mt. Taylor, New Mexico, near Fillmore, in Central Utah, and at several points in southern Utah. The specimen which I exhibit is from Fillmore. While it was molten and moving, it rubbed against a lava mass already congealed, and then cooled quickly enough to retain the surface produced by the friction. The perfect preservation of this surface, which is like that which the trowel gives to thick mortar, will show you how little the lava has yielded to meteoric agents.

4. In some places the lightest scoria has not been appreciably abraded, although exposed to the action of the elements. If you examine this specimen, you will perceive that it is a light, spongy mass, a mere hardened froth, the bubbles of which are separated by papery partitions, while the exterior is surrounded by a smooth skin of equal thinness. Erosion to the depth of a hundredth part of an inch would penetrate this skin and expose the vesicular interior. Yet this specimen, which I broke from the top of a cratered cone, near Fillmore, has been exposed to whatever storm, and rain, and frost have visited that country since it was formed, and its outer film is unimpaired.

5. At a locality near Mount Taylor in New Mexico, where a lava stream (one that was visited and noted years ago by Marcou and Newberry) is crossed by a small creek, the Ojo del Gallo, the running water has cut into the compact lava less than an inch.

By these facts, the most cogent of which is, perhaps, the freshness of the frothy scoria, I am led to believe that the antiquity of the last eruption within the United States, is measured by hundreds more probably than by thousands of years, so that it is to be held as essentially a modern event. And by the comparison of these late eruptions with earlier outflows of a similar character, some guess may even be hazarded with reference to the future. In southern Utah and northern Arizona is a broad region of strata dipping gently to the northward. A portion of the region is floored by Carboniferous, and another portion by Triassic rocks, and the two are separated by a long escarpment or line of cliffs, constituted by the edge of the Triassic sandstones. An erosion of infinite slowness is carrying these cliffs back toward the north and thus increasing the Carboniferous area at the expense of the Triassic. If we picture this cliff, for a momentary illustration, as a shore, then sixty miles to the south of the shore is an island of Triassic rock, fifteen hundred feet high, and the island has been preserved from the degradation to which the surrounding country has been subjected, by a protecting mantle of basalt.[1]

The portion of this history with which we are at present concerned is the eruption of basalt, antecedent to the erosion of not less than fifteen hundred feet of Triassic strata, accomplished progressively over a belt sixty miles broad. A series of later basaltic outflows dot the Carboniferous plain, and one in particular has burst from the lower slope of the Triassic escarpment in its *present* position. The difference in age of the first and last of these basalts—and this is a minimum estimate of the duration of the (basaltic) volcanic epoch, in that region—is measured by the sixty miles of denudation. If now we contrast the length of this epoch, as thus measured, with the recency of the Fillmore and other lavas, measured by the scarcely tarnished scoria, we see that the age of the latest eruption is but the differential of the age of the earliest; and, knowing that all volcanic activity is rhythmic or intermittent, it is more logical for us to believe that we live in one

[1]The discovery of this Triassic island, the Uinkaret Mountains, and of the manner of its preservation, was first made by Prof. J. W. Powell.

of the intervals between outbursts, than that the phenomena have ceased. I see no reason to suppose even that the end is near, and shall feel no surprise when the fires burst forth anew.

THE ADVANTAGES OF THE COLORADO PLATEAU REGION AS A FIELD FOR GEOLOGICAL STUDY. By G. K. GILBERT, of Rochester, N. Y.

ABSTRACT.

THE country which extends from "the Plains" to the Pacific ocean is a mountain country — a region of corrugation — ridged with an unnumbered succession of mountain ranges, which rise with the same disparity of size and persistence of recurrence that belong to the waves of the ocean. The mountains are complex in composition and structure, and the strata that appear in them are broken and folded and tilted. In the midst of this ocean of disturbance is an island of calm. Surrounded on every side by the mountains is a district in which the strata are nearly horizontal, and dislocations have been comparatively slight. Its topographical features are as distinct as its geological, and it lies, within the province of mountains, a province of plateaus or tables. It is known to those who are now engaged in its exploration, as the Colorado Plateau Region, or the Colorado Plateaus. It comprises adjacent portions of Utah, Colorado, Arizona and New Mexico, and has an extreme length, north and south, of 450 miles, with a width of 350. Its character was first recognized, and the term "plateau" applied to it, by Ives and Newberry, who touched its southern margin nearly twenty years ago. More recently it has been traversed and studied by Newberry, during what is known as the Macomb expedition, and by Powell in his exploration of the Colorado River; and, when the results of these labors shall have been published, much will be known of its history. The Wheeler expeditions of the war department, of which I have been a member, have accomplished a great deal toward the determination of

the boundaries of the region, and have made additional contributions to a knowledge of its structure; but the results are not yet printed. Pending the publication of all this material, I wish to call the attention of the Association to the peculiar facilities which the region affords for certain lines of geological investigation, and to the value of the results that are to be anticipated.

1. The greatest facility for examination is given by the aridity of the region. Save on the highest tables, there is no timber; and over great areas there is no soil. The rocks are naked, and the geologist, in travelling, even rapidly, can always tell whether he is ascending or descending in the geological scale. This aridity is no less a characteristic of the mountain regions to the west and south, but there the general level is so low that the drainage is imperfect, and the detritus from the mountains accumulates in the intervening troughs, thus cloaking half the surface. The Plateaus are so elevated that their drainage system is thorough, and the waste from their surface is carried beyond their borders.

2. It is a consequence of the dryness of the region that frost, and solution, and chemical reaction, are less important factors of denudation than is abrasion; and, since the tables are elevated, and there is great descent along the lines of drainage, an importance by magnitude is given to all the phenomena of erosion. The field is hence peculiarly adapted to the study of erosion by running water, which there exhibits its varied phases, and is almost perfectly differentiated from other agencies.

3. Few countries—perhaps no country—can afford equal facilities for the study of stratigraphy. Take, for example, the region of the Grand Cañon of the Colorado, and consider it merely as a field for the study of the Carboniferous rock system, there more than 3000 feet thick. Throughout a tract seventy-five miles broad, and twice as long, the drainage lines are from 1000 to 6000 feet below the general plateau. The gorge of the Colorado traverses it from end to end, and tributary gorges ramify on both sides, so as to gash the strata with a thorough system of natural sections,—sections too that demand no detailed scrutiny for their appreciation, but appeal to the eye like diagrams on a printed page. A stratum can be kept in sight literally through hundreds of miles of outcrop, and its modifications can be traced not merely on a single *line*, but on such a *plexus* of lines that it can be in-

timately known through a broad *area*. Conformity and non-conformity can be demonstrated beyond all cavil, for no break of deposition could fail, in this stupendous exposure, somewhere to betray itself. The country is, indeed, not easy to traverse, but it has been abundantly demonstrated that it is not inaccessible. It is true that the walls of the cañons, or gorges, are difficult to scale, but the public needs to be disabused of the idea that they are sheer precipices from brow to base. They are everywhere terraced on a grand scale, descending in an alternating series of easy slopes and abrupt cliffs, that mark respectively the soft and hard strata of which they are built. The sloping terraces are as persistent as the cliffs, and open pathways for lateral exploration, between the somewhat infrequent points at which the cliffs can be scaled.

4. Finally, and, to my mind, chiefly, the Plateaus offer a most notable contribution to the study of mountain structure. They are contrasted with the adjacent areas of corrugation, not by the absence of the dislocations that characterize those areas, but by possessing them only in an elementary condition, and in slight degree. All through the province of little disturbance runs a system of faults and monoclinal folds,—dislocations ranging from a few hundred to two or three thousand feet in amount. The monoclinal fold is the precise geological equivalent of the fault, and is in form the half of an anticlinal fold. If, in traversing a plain floored by a continuous level stratum, the geologist comes to the base of a cliff, and, on climbing the cliff, finds himself upon the same stratum, stretching level and continuous before him, he knows that he has crossed a fault, the throw of which is equal to the height of the hill. But if, instead of a cliff, he encounters a hill of rounded profile, and finds, in climbing it, that he is walking all the time on the same stratum, bent concave at the foot of the hill and convex at the top, and continuous with the level portions that floor the plain below and the plain above, he will have passed a monoclinal fold. And then, if, changing his course at right angles, he follow the brow of the hill, and find that, at some place, the monoclinal fold changes to a fault (as he can find at a score of places in Utah and Arizona), he will recognize the equivalence of the two structures. They result, alike, from a change in the relative height of two portions of a mass of horizontal strata; and either may occur, according as the strata are fractured, or merely flexed, in the dislocation.

Some of the structure lines of the Plateaus, characterized by these faults and folds, are very persistent; one has been traced for more than two hundred miles. They are spaced and arranged like the mountain ridges of Nevada. They are, indeed, an incipient, embryotic mountain system; perhaps a system arrested in its development, but more probably a system now growing, and, in either case, endowed with the interest that ever belongs to germs. The monoclinal fold, barely recognized by geologists heretofore, but here the predominant structure, is one of the simplest elements of corrugation, and the fruit of its careful study cannot fail to be of great import to the student of dynamical geology.

Barren and naked, gashed by deep cañons, carved by a selected, typical erosion, revealing its inmost anatomy to the student, and pregnant with the germs of mountains, the region of the Plateaus is indeed fit to be called "the paradise of the geologist."

On the Equivalency of the Coal Measures of the United States and Europe. By C. A. White, of Brunswick, Maine.

Few, if any American geologists, doubt that, according to the accepted means and methods of determining such questions, the coal-measures of the United States are chronologically equivalent with those of Europe.

Several European geologists, however, who have at various times visited and made collections from the coal-measure rocks of our country at a few isolated localities, or who have studied collections sent to their countries from our own, have expressed the opinion that our coal-measure strata are chronologically equivalent with their lower or subcarboniferous limestone. We had hoped that this erroneous view would have been corrected long ago, but so lately as April of the present year, Dr. L. G. De Koninck, in a communication to the Academy of Science at Brussels, again expressed the same views. On this occasion he stated

that a majority of the species in a collection sent by Mr. Worthen from the coal-measure rocks of Springfield, Illinois, were identical with those occurring in the lower or subcarboniferous rocks of various European localities, and to which horizon he referred them. Professor Geinitz, of Dresden, reached a similar conclusion regarding some of the collections made by Professor Marcou in Nebraska in 1863, from upper coal-measure rocks, while he referred other species of the same collections to the Permian period. We have in the latter case the anomalous example of a reference of fossils of one well-defined epoch, that of the upper coal-measures, to both the lower carboniferous and the Permian. Such facts themselves show, if others were wanting, that the views of these gentlemen are obtained from a European standpoint, with little knowledge of the general stratigraphical relations of the rocks, as they exist in our country.

It is true that this mingling of what are regarded in Europe as Permian, coal-measure and subcarboniferous types, in the same strata, does really occur in our coal-measure rocks.

It is also freely admitted that synchronism of strata, in widely separated localities, cannot be ascertained with absolute accuracy, but I wish to present a brief summary of the facts which are relied upon to prove the equivalency of the coal-measure rocks of our country with those of Europe, in the sense that equivalency of formations is generally understood among geologists.

In Europe, the area now occupied by the true coal-measures was, during the deposition of the formation, much less frequently invaded by the sea than the great area occupied by our coal-measures. There, comparatively few marine fossils are found in the coal-measure strata, but in ours marine fossils occur in almost every stratum throughout the whole series. There, the entire fauna was changed at the completion of the subcarboniferous, and the introduction of the coal-measure epoch. Here, on the contrary, the sea occupied *some part* of our coal-measure area, shifting slowly here and there, from the beginning to the end of the epoch. Hence a large proportion of the fauna of the subcarboniferous period, especially the Brachiopoda, continued to exist through the coal-measure epoch. Thus we find that not only do subcarboniferous *types* occur in the coal-measures, but some of the *species* also pass up from our own subcarboniferous to coal-measure strata, and the latter contain, in addition to these, species that

are identical with those found only in the subcarboniferous of Europe.

Notwithstanding these facts, we do find that other faunal and the floral characteristics point unmistakably to an equivalency of American and European coal-measures. Take, for example, the articulate fauna: — *Bellinurus* of Europe is represented by *Euproöps* of Illinois; *Gampsonyx* of Europe, by *Acanthotelson* and *Paleocaris* of Illinois: *Anthrapalæmon* of Europe, by the same genus in Illinois; *Euphoberia* of England, by the same genus in Illinois; the oldest known scorpion *Cyclopthalmia* together with some true spiders of Europe, by *Eoscorpius* and the spider *Arthrolycosa* of Illinois. These facts are strikingly presented and illustrated in Dana's new edition of his manual of geology, pages 334 and 350.

Again Dr. Newberry and Mr. Lesquereux, our best authorities on fossil plants, find that at least fifty per cent. of the species of plants discovered in our coal-measure strata are identical with those of Europe. These gentlemen have not only studied the plants from the coal-measure rocks from numerous and widely separated localities in North America, but they have carefully compared them with those of Europe.

But it is not only upon these and numerous other similar facts, nor by any means upon the presence of coal in our coal-measures, that American geologists rely in referring them to the horizon of the European coal-measures. We have, in addition to these, the great fact that the entire physical structure and order of succession of our palæozoic series of formations is the same as it is in Europe. We have the great Silurian system beginning with the Primordial and represented also by the other succeeding subordinate divisions. The Silurian is succeeded here, as in Europe, by a well defined system, corresponding in all respects with the Devonian of Europe. This in turn is succeeded by a group of strata corresponding, not only in the leading characteristics of its fauna, but also in its general lithological features with the mountain limestone or lower carboniferous of Europe. Upon this group in our country are found the conglomerate and grits which represent the millstone grit of Europe. Upon this formation succeeds a great series of sandstones and shales, with a little, usually impure limestone, and also the coal beds, all constituting our coal-measures. This formation corresponds in general lith-

ological features with the coal-measures of Europe, and is the one which contains not only the fossils from Illinois that Dr. DeKoninck has referred to the mountain limestone, but also those articulates and plants mentioned in a previous paragraph. Westward from the Mississippi River however, our lower or productive coal-measures pass gradually upward into a division, containing little or no coal and composed largely of limestones. This division is generally designated as the upper coal-measures, but is often not clearly separable from the lower division, and in all cases its prevailing types are those of the lower coal-measures. It is true that in these upper coal-measure rocks are found many forms that European geologists call Permian, but the same species are also found far down in the coal-bearing strata that contain, as before said, not only the floral and articulate forms before referred to, but those also that Dr. DeKoninck refers to the mountain limestone. The last named forms are especially abundant in our upper coal-measures where they are associated with Permian types. It has been my good fortune to trace minutely these strata from the millstone grit to the upper coal-measures, from Illinois to Nebraska and Kansas, and I have seen them to pass, without physical break or unconformability, through the rich coal-bearing strata into the upper coal-measures and thence into beds in Kansas which contain almost exclusively Permian types.

It is doubtless this mixture of subcarboniferous, coal-measure and Permian types in our formation that has puzzled European geologists, but in view of the foregoing and other paleontological facts, which might be mentioned, together with the physical characteristics and the order of geological succession, there can be no doubt that we must necessarily regard the coal-measures of Europe and America as geologically equivalent.

Indeed if our coal-measures do not represent the coal-measure epoch of Europe, then there is no place in our geological series for such representation.

The Disintegration of Rocks and its Geological Significance.

By T. Sterry Hunt, of Boston, Mass.

ABSTRACT.

This subject the speaker had briefly noticed in a communication to the Association last year, on the geology of the Blue Ridge. The change of the rocks in question is a chemical one, which is most obvious in the case of crystalline rocks; the feldspar loses its alkalies and part of its silica, being changed into clay, and the hornblende its lime and magnesia, retaining its iron as peroxide. From this results a softening and decay of the rocks to greater or less depths, so that while the beds still retain their arrangement, and are seen to be traversed by veins of quartz and of metallic ores, they are often so much changed to depths of a hundred feet or more as to be readily removed by the action of water. This phenomenon is well seen in the crystalline rocks of the Blue Ridge, and not less remarkably in those of Brazil, where it has been noticed by many observers, among the latest of which is Professor Hartt. Darwin, who long ago described it, imagined the change to have been effected beneath the sea, but according to the speaker it has been a sub-aërial process, which has been at work during past ages, when the composition of the atmosphere and the climatic conditions differed from those of to-day, and when carbonic acid, aided by warmth and moisture, abounded. He connected it with that slow purification of the atmosphere which, from very early times, has been going on. The alkalies and lime and magnesia, set free in this process, absorbed the atmospheric carbonic acid, and the carbonates carried down to the sea in a dissolved state gave rise to limestones, dolomites, and common salt. Such a process of decay was already active at an early period, and, from facts observed in Missouri by Pumpelly, had affected the iron-bearing feldspar-porphyries at the commencement of palæozoic time. It was, according to the speaker, from the washing down of the thus decomposed crystalline rocks that all the clays and sands which had gone to build up the sediments of our vast beds of palæozoic and more recent rocks had been either directly or indirectly derived. This had already been pointed out by Lyell for the tertiaries of the southern states. He thought it probable that the process of decay had gone on with

decreasing energy to our times, though it is now insignificant in its action, owing to changed atmospheric conditions.

The speaker drew a picture of North America in past geological ages. The frequently taught notion of the growth of our continent southward and outward from a nucleus in the vicinity of the great lakes has no foundation in fact, and the study of the uncrystalline sedimentary formations tells a different story. The great palæozoic basin was to the east and west, as well as the north, surrounded by areas of decaying crystalline rocks. Those of New England, of the Blue Ridge, and of the crystalline area to the east of it are the remains of a great disintegrated and wasted continent, whose ruins have built up the uncrystalline rocks to the westward of it, as well as the sediments along the eastern and southern borders of the United States. Up to a comparatively recent period, the hills of New England, eastern New York and New Jersey, were probably, like those farther southward along the Blue Ridge, deeply covered by the products of their own decay, and from these were derived the clays, as well as the brown iron ores which are found along the base of the Blue Ridge and its northeastern continuation.

It was during the glacial period, which the speaker considered to have been one of submergence and subsequent gradual uplift of northeastern America, at which time it was exposed to the action of local glaciers and to the iceberg-drift of the polar current, that the final removal of this decayed covering from our hills had taken place, while farther southward the mountains beyond the reach of this denuding action still retained to-day their covering of decayed rock. A similar condition of things is to be seen in northwestern Minnesota, where, according to White, the decayed crystallines have escaped denudation. The process of decay in the more massive and granite-like rocks had often been incomplete, and working from natural joints had left unchanged nuclei of hard rock which, when erosion took place, remained as rounded masses or bowlders; a point which has been well brought out by Mr. Barbank from his studies in North Carolina, and throws much light on our northern drift-deposits.

A profound decay had also affected the hard palæozoic limestones, from which the carbonate of lime had been dissolved, leaving a porous, rotten rock behind. This, as Dawson has shown, is well seen in the impure argillaceous Trenton limestone

near Montreal, which, in localities protected by trappean dikes from the eroding action which came from the northeast, is found deeply decayed, while elsewhere, near by, its hard surface is worn down and glaciated. Examples of a similar local exemption of the decayed crystalline rocks from erosion are not wanting in New England.[1]

The speaker alluded, in closing, to a process of mechanical disintegration, without chemical change, which in past ages had broken up undecayed crystalline rocks to form breccias and conglomerates. These are seen locally, from the ancient porphyry-conglomerates of the Lake Superior copper-mines to very recent deposits, and a remarkable example is met with in the beds of granitic material, which, in a recemented state, make up parts of the red sandstone of the Connecticut valley. The slow breaking up of many crystalline rocks by the action of frost had been suggested by Dawson as a potent agent in the production of such material, and the speaker conceived that this, in the present state of our knowledge, was the most probable explanation of its origin.

CREMATION AMONG NORTH AMERICAN INDIANS. By JOHN L. LECONTE, of Philadelphia, Pa.

THE object of the present note is merely to record the fact, that among the many different methods of paying the last tribute of respect to deceased members of the tribe, which are now practised by the native races of North America, cremation is not entirely omitted.

In December, 1850, while enjoying the hospitality of the detachment of the 2nd U. S. Infantry, which at that time established Fort Yuma, the military post at the junction of the Colorado and

[1] An example of this is seen in Kent in Connecticut, and also in North Adams, Massachusetts, where the tunnel lately made through the Hoosac Mountain shows that the gneiss rock at its western base is completely decomposed to a depth of 200 feet or more; while on the summit of the mountain are seen worn and glaciated surfaces of the same rock in an undecomposed state.

Gila Rivers in California, I availed myself of the kind offer of Mr. Jordan, one of the owners of the ferry near the post, to make with him an exploration of the river below the junction.

Starting in a small flat boat, which he generously sacrificed for the purpose, with a Yuma Indian, who had a feeble knowledge of Spanish, as guide and interpreter, we floated down with the current of the river, making, by the aid of a solar compass, a rough survey. On the afternoon of the third day we arrived at the lowest village of the Cocopa Indians, who are the next tribe south of the Yumas. Below that village we were told that the spring tides widely overflowed the banks of the river, and that if we went farther, the softness of the mud might seriously hinder our return.

The next day I learned from the guide that an old man had died in a village near the east bank of the river, and that the body was to be burned.

Never having heard before that this custom existed in North America, we eagerly availed ourselves of the opportunity of seeing the interesting ceremony. Crossing the stream in our flat boat, we arrived, after a walk of a couple of miles over the river bottom and adjoining desert, at the late residence of the deceased.

A short distance from the collection of thatched huts which composed the village, a shallow trench had been dug in the desert, in which were laid logs of the mesquite (Prosopis, and Strombocarpus), hard and dense wood, which makes, as all western campaigners know, a very hot fire, with little flame, or smoke. After a short time the body was brought from the village, surrounded by the family and other inhabitants, and laid on the logs in the trench. The relatives, as is usual with Indians, had their faces disfigured with black paint, and the females as is the custom with other savages made very loud exclamations of grief, mingled with what might be supposed to be funeral songs. Some smaller faggots were then placed on top, a few of the personal effects of the dead man added, and fire applied. After a time, a dense mass of dark colored smoke arose, and the burning of the body, which was much emaciated, proceeded rapidly. I began to be rather tired of the spectacle, and was about to go away, when one of the Indians, in a few words of Spanish, told me to remain, that there was yet something to be seen.

An old man then advanced from the assemblage, with a long

pointed stick in his hand. Going near to the burning body he removed the eyes holding them successively on the point of the stick, in the direction of the sun, with his face turned towards that luminary, repeating at the same time some words, which I understood from our guide was a prayer for the happiness of the soul of the deceased. After this more faggots were heaped on the fire which was kept up for perhaps three or four hours longer. I did not remain, as there was nothing more of interest, but I learned on inquiry, that after the fire was burnt out, it was the custom to collect the fragments of bone which remained, and put them in a terra cotta vase, which was kept under the care of the family.

The ceremony of taking out the eyes, and offering them to the Sun, seems to indicate a feeble remnant of the widely diffused Sun worship of former times, but when introduced, or whence derived, I could not learn. The subject appears to me an important one, and to deserve attention from those who are so situated as to procure further information.

None of the Cocopas whom I met had sufficient knowledge of Spanish to enable me to communicate easily with them, so that I learned little of their history or habits, during the two days that I remained among them. I however wrote down their numerals and a few other words, which were sufficient to confirm the information I afterwards obtained.

On a subsequent journey along the Gila to Tucson and other towns, then belonging to the Mexican state of Sonora, I passed through the villages of the Coco-maricopas who, as is well known to all of my hearers, live in a semi-civilized condition, in close bonds of union with the Pimos, on the banks of the Gila.

I was led by the similarity of language, as well as by the resemblance in name, to suspect that this tribe was related to the Cocopas of the lower Colorado. On enquiring, I was told by one of the chiefs, Francisco Duk, that they still preserved a tradition of the former connection of the two tribes. Many years ago, in search of more extensive lands, the Cocopas had separated from them, and gone westward, settling on the banks of the Colorado, below the confluence of the Gila. Visits were occasionally made to their villages by their kinsmen from the Colorado, and in fact, I had met on my journey a small party of Cocopas returning from the Maricopa villages.

The Maricopas are now completely identified in interests and habits with the Pimos, and if they practised cremation when they first entered the Gila valley, the usage has long since become obsolete.

Commercial intercourse between the Indians of these interior valleys and those of the Californian Gulf must have also taken place centuries ago, when a higher form of semi-civilization existed along the Gila. For not many days afterwards while examining the famous Casas Grandes or Casas Blancas, as they are more usually called, I found shells of the genera Oliva and Conus, which had been brought from the Gulf. Small ornaments of turquoise, similar to the variety found near Santa Fé, New Mexico, occasionally occur and are greatly prized by the Indians.

An Instance of Replacement of Injurious Insects by Human Agency. By John L. LeConte, of Philadelphia, Pa.

ABSTRACT.

This paper simply notes the fact that in Philadelphia, and probably in other cities, the Geometride, *Ennomos subsignaria*, which was formerly very injurious to the shade trees growing in the streets, has been exterminated by the European sparrows, introduced for that purpose. With the disappearance of the Geometride, a Noctuide, *Orgyia leucostigma* commenced to increase, and has now in some streets become almost as great a nuisance as the *Ennomos* had been. The larvæ of the Orgyia, whether protected by some disagreeable odor, or more probably by the stiff hairs with which they are covered, are not eaten by the sparrows, and therefore increase without molestation. It is recommended, that the cocoons, which in the cities are mostly made in exposed places on walls and fences, should be destroyed, and the trees surrounded with girdles of tin, or tar, or some other material which caterpillars cannot cross, so as to prevent the extension of the pest to other trees. As the female of the Orgyia has no wings proper for flight, this would seem to be an effective way to confine the depredations to narrow limits.

Trap Rocks of the Connecticut Valley. By Edward S. Dana, of New Haven, Conn.

During the past three months Mr. G. W. Hawes of New Haven and myself have been engaged in the study of the trap rocks of the Connecticut Valley, and in that time we have visited a large number of the more prominent or interesting dikes. We propose, ultimately, to investigate the subject in all its bearings—the varieties of the rock, their distribution and relation to each other; the changes produced in the trap, and in the rock through which it was ejected at the contact surface, and the paragenesis of the various secondary minerals in their relative succession; and finally, the alteration caused by surface decomposition. A subject so broad, however, must necessarily require time, and as other duties may considerably delay its completion and final publication, it occurred to me that it might be worth while to offer a few remarks here in regard to what has already been done. This embraces the results of a preliminary microscopic examination of the rocks from more than a hundred localities, by means of their sections prepared for the purpose, a method of investigation of the greatest value in the case of such rocks as these, by means of which the mineral constituents may be definitely determined, instead of being guessed at from the results of a chemical analysis.

The trap belongs to an extensive series of fissure eruptions connected with the Mesozoic sandstone, not only of the Connecticut Valley but also of Nova Scotia, and of New Jersey, Pennsylvania and North Carolina. These dikes of trap, however, are nowhere displayed to such extent and in such number as in our vicinity—in Connecticut—where they have been studied topographically and mapped with a marvellous degree of accuracy and minuteness by Percival. The trap rock, microscopically investigated, shows throughout a crystalline texture. It is made up of pyroxene, labradorite and magnetite, with also occasionally some chrysolite and apatite. Chlorite is often present as the result of local change. The pyroxene sometimes, in coarse-grained varieties, runs off into long-bladed prisms, somewhat resembling hornblende, which name has in consequence often been given to it; the pyroxene is the first constituent to suffer from surface alteration. The magnetite is most commonly in irregular masses, but some-

times shows curious and beautiful arborescent crystallized forms, frequently observed elsewhere in similar rocks.

It is interesting to observe that these peculiar dendritic forms have been found as yet only in the more hydrous rocks, although future study may not confirm this. The feldspar shows before the microscope its triclinic character; and an analysis of the rock by Mr. Hawes proves that it has the composition of labradorite. This analysis gives for the rock exactly the composition of a doleryte, and it must receive this name, as it has generally done hitherto, though in this we go counter to foreign usage, according to which doleryte is a younger rock, not older than the Tertiary. It is important to observe that the rock, as it contains no hornblende, is not dioryte, though that name has also been given to it.

Turning, however, from what may be called the normal rock, for example, that analyzed by Mr. Hawes just referred to, containing almost no water, we find other varieties containing a considerable amount, and here the microscope comes to our assistance, making it possible to extend our observations over a wide range in a very short time. We find that the trap which has come up through the older crystalline rocks is most of all free from hydrous minerals, or any alteration, having a fresh, vitreous look on the fracture. This is true but not to quite so great an extent of trap from the West Rock range, from East Rock near New Haven and other points south and west. As we go from West Rock eastward, however, the character of the rock changes; it loses its freshness, is dull in fracture, and is often amygdaloidal. The change is due to the hydrous character. The rock from the Saltonstall ridge, for example, contains 3 to 4 per cent. of water. This trap is throughout more or less green under the microscope, containing a chlorite sometimes in plates, seemingly made at the expense of the pyroxene, sometimes in cavities, these last often entirely invisible to the unaided eye. This general character belongs to the trap more or less decidedly from East Haven north, following the eastern range through the Durham and Middletown Mountains; and its continuation north to Mt. Tom; it is also true of a large portion of the Meriden range. In addition to this massive, though generally chloritic trap, which makes up most of the great ridges as seen on Percival's map, we find also another variety quite different—this

is light green in color, soft, very hydrous, and has its feldspar as well as pyroxene very much altered. It is most characterized by its amygdaloidal structure, the cavities being very numerous, filled with calcite or chlorite, quartz, and sometimes datolite or analcite, or curiously enough with bitumen; this is true of some dikes west of Hartford. This amygdaloid, moreover, is found in a series of low subordinate ridges parallel to, or concentric with, some of the most prominent ranges—for example at the Meriden Hills. This forms one of the most curious phenomena connected with this subject, and is not easy of explanation. The fact, just alluded to, that the amygdaloidal and massive trap occur in the same dike, seems to shut out the idea that these subordinate ridges are of a different age; but if, on the other hand, we accept the idea suggested by Prof. Dana in a recent article that the moisture in igneous rocks found access to them in process of eruption, we may regard all these as changes wrought in what was originally essentially the same material by local causes in introducing moisture. As we should then expect, the trap in the crystalline rocks is anhydrous, as before remarked. I leave the subject here, however, since until many more facts have been collected and the work of the microscope has been supplemented by that of the laboratory under the hands of Mr. Hawes, any definite conclusions will be unsafe. In a few individual cases the Triassic trap from Nova Scotia, New Jersey, Pennsylvania, and North Carolina has been examined, and as far as microscopic structure goes it is hardly to be distinguished from the trap of Connecticut Valley.

Remarks were also made as to the secondary minerals in trap, and the metamorphic effects of the heated trap on the adjoining sandstone.

Observations on the Mesozoic of North Carolina. By W. C. Kerr, of Raleigh, N. C.

In comparing the Triassic and Jurassic formations of the Atlantic slope in its different outcrops, from Vermont to South Carolina, there are several interesting points of difference and of contrast observable.

In the first place, while the formation is monoclinal northward the dip being sometimes east and sometimes west, in North Carolina there are two opposite dips constituting a distinct anticlinal.

In the second place, while its single outcrops northward are wider (about twenty miles in Connecticut, for example, and rarely reaching the half of that south), the entire breadth of country between the two anticlinal fringes in North Carolina on Deep River and Dan River, which was doubtless formerly covered by it, is at least four times as great as its greatest breadth northward.

In the third place, while the formation is everywhere very much cut up by trap dikes, these dikes occur on a vastly larger scale in Connecticut than in North Carolina, in the one case forming mountain chains of considerable extent, and in the other, limited to narrow ledges of a few feet or rods in breadth, and rarely reaching a mile in continuous longitudinal extent: and in the latter case they are generally transverse to the strike, while in the former they are prevalently coincident with it.

In the fourth place, the formation is very barren of organic remains in its northern outcrops, and highly fossiliferous in North Carolina, containing important and valuable beds of bituminous coal.

Besides these points of difference, there are several other notable facts which I restate here for the sake of emphasis with a view to certain important conclusions which they seem to suggest. First, the formation, narrow as it is, is continental in linear extent, having a length of more than six hundred miles, and has a depth of several thousand feet, according to probable estimates, not less than 3000 feet in many places. It is very slightly compacted for the most part, and very readily subject to denudation, its remnants everywhere lying in trough-like depressions, having evidently suffered a much greater amount of waste from erosion than the enclosing and underlying formations. We have some hint of the amount of this waste in the fact that the whole thickness of the formation has been swept away from the broad-backed swell which separates the two narrow borders of it in North Carolina, except one or two insignificant outliers near the northern border of the state, and in the occurrence of a third outcrop eastward of the Deep River belt and several miles from it, of the same eastwardly dip, thus adding many thousand feet to the former estimates which were based upon the last named outcrop. Again in North

Carolina the heaviest conglomerates are found along the eastern margins of the formation, the embedded bowlders, often more than a foot in diameter, being traceable to the older rocks of the hills lying several miles to the eastward.

Now it is highly improbable that a formation of so great length and depth should not have had a much greater original breadth, but if the restorations are made which the stratigraphy imperatively requires (the dips of the eastern fringes being about 20°, often 30° in North Carolina), it is evident that there must have been an inland sea or great bay, not less than one hundred miles wide toward the south, probably narrowing toward the north but still of great breadth — something like the Chesapeake — with a high swell of land, consisting of the primary rocks, lying between it and the Atlantic, from which, in large part, its material was derived.

And this may have been the last of that eastward and northeastward continental area required by Dr. Hall's (and it seems very rational) theory of the formation of the palæozoic rocks of New York and of the Appalachians and the northwest, which are so distributed as to indicate an eastern origin.

Studies in the Formation of Mountains in the Sierra Nevada, California. By John Muir, of Oakland, Cal.

ABSTRACT.[1]

These studies in mountain building refer particularly to that portion of the Sierra which is embraced between Lat. 36°, 30′ and 39°. It measures about two hundred miles in length by about sixty in width, and attains an elevation along the axis, from eight thousand six hundred to nearly fifteen thousand feet above the sea.

[1] This paper was accompanied by a large number of illustrations, and it is greatly to be regretted that the funds of the Association would not permit of their being reproduced as cuts. This has necessitated the abbreviation of portions of the communication, and in justice to the author I must state that he has not read proofs owing absence from Oakland, where they were sent.—Editor.

All the individual mountains distributed over this vast area, whether the precipitous ranks marshalled along the summit, or the more beautiful and highly specialized domes and mounts dotted over the broad undulating flanks, or the immense bosses and angles projecting horizontally from the sides of valleys, have all been brought into relief during the glacial epoch, by the direct mechanical action of the ice-sheet and the glaciers into which it afterwards separated.

The chief phenomena presented are: 1. *Scratched and polished surfaces.* 2. Moraines. 3. Moutonnéed rock forms and sculpture in general, as seen in valleys, ridges, lake basins and separate mountains.

The polished surfaces occur in the summit and upper half of the middle region, in irregular patches, some of which are several acres in extent, and though they have been subjected to corroding influences for thousands of years, their mechanical excellence is such that they still reflect the sunbeams like glass. The most extensive areas are found in the upper half of the middle region where the granite is most solid in structure and contains the greatest quantity of silex. They are brighter, and extend farther down on the axis of the range on the *north sides* of valleys, because when wet by corroding rains and snows they are much sooner dried again, the north sides receiving sunshine, while the south reposes in shadows.

The lowest patches occur at elevations of from three thousand five hundred to five thousand feet above sea level, and thirty to forty miles below the summits, upon the driest and most enduring portions of vertical walls, where they are protected from the drip and friction of water; also on hard swelling bosses on the bottoms of valleys, where they are kept dry by bowlders.

In the summit regions, small active glaciers still linger, which present moraines of every kind in process of formation. Those of the ancient glaciers are clad with forests and extend down through the middle region of the range. In the lower region, polished surfaces are entirely wanting. So also are moraines, though the material which once composed them is found scattered, washed, crumbled and re-formed over and over again until its glacial origin would scarcely be suspected, unless we trace it back into the undisturbed moraines of the middle region.

All of the magnificent forests of the Sierra are growing upon mo-

raine soil, which, as yet, has suffered little change since it was first eroded and outspread for them by the ancient glaciers.

The most imperishable of the glacial phenomena under consideration, are the *valleys*, *ridges* and *rock masses;* the *trends*, shapes and geographical position of which, are specifically glacial. Yet even these are considerably obscured by post-glacial weathering, and a growth of forests. The glacial phenomena therefore brighten and become more legible as we ascend to the fresh work of the living glaciers on the summits.

Rock and snow avalanches have produced marked changes, but these are very much restricted in their range, and have done little towards altering the glacial physiognomy even of the localities where they prevail.

On the slow and gentle withdrawal of the ice-mantle, many a weak tower and impending brow were shattered by the first severe post-glacial earthquake, traces of which are still conspicuous in every sheer walled valley and around the bases of the summit Alps. *To this earthquake* alone, which occurred at least three centuries ago, nine-tenths or more of the cliff taluses are due.

A similar but less severe shock, which I had an opportunity of witnessing, produced another series of taluses in March, 1872.

The post-glacial streams of the Sierra are all young and have not yet flowed long enough over the brilliantly polished bottoms of the glacier valleys to make definite beds. The highest flood marks in the middle and upper regions seldom exceed seven or eight feet above the present level at ordinary stages, and the actual erosion effected is not more than half that amount. *Atmospheric weathering has done more to blur and degrade the glacial features of the Sierra than all other agents combined.*

Throughout the whole of the upper and middle region the quantity of this degradation can be very nearly approximated, by measuring down from the edges to the original striated surfaces. Bowlder pedestals occur quite frequently in the middle granite region, some with their protecting bowlders still in place, others naked, the bowlder having rolled off. All the oldest ridges and slopes are bowlderless, nature having leisurely rolled them off, giving each a whirling impulse as it fell from its stool once in hundreds or thousands of years.

This brief survey of the various forces incessantly or occasionally in action, wasting the mountain surface, would at first lead us

to suppose that the total amount of degradation must be enormous. But on the contrary so indestructible are the Sierra Rocks, and so short is the period through which they have been exposed to these agents, the general result is comparatively insignificant.

The polished striated areas, constituting so considerable a portion of the upper and middle region, have not yet been degraded to the one-hundredth of an inch. Farther down, measuring tablets abound, bearing the signature of the ice sheet, while the amount of torrent erosion is certainly estimated by measuring the depth of their channels below the unchanged glaciated surfaces lining their banks. Still farther down, where the polished surfaces disappear, we have other valuable data for our estimates in the height of pot holes drilled into the walls of gorges, and by the form of the bottoms of the valleys containing these gorges.[2]

Summing up these results, we find the average quantity of post-glacial denudation in the upper half of the range, embracing a zone twenty-five or thirty miles wide, *probably does not exceed a depth of three inches.* That of the lower half has been much greater, probably several feet. Nevertheless it is certain that the main features have not been radically altered. *The physiognomy* of the range from base to summit is strictly glacial. Rivers have traced shallow wrinkles, avalanches have made scars upon it, and forests have bearded it, but the change is not greater than a few years would make upon a human face exposed to winds and storms and the changing atmosphere.

The multitudes of lofty mountains, distributed along the summit of the portion of the Sierra under consideration, are almost wholly unexplored. They have been entered hastily at a few points adjacent to trails, and a few of the more accessible have been ascended, such as Mts. Dana, Lyell, Whitney, and Tyndall, while the grand wilderness of mountains, from whose fastnesses the chief tributaries of the San Joaquin and King's Rivers take their rise, has been mapped from a distance without any attempt at detail. Their echoes are never stirred by the hunter's rifle whether Indian or white man, for, excepting the wild sheep, there is no game to tempt him even to the lakes and meadows that lie around their bases, while their glaciers, avalanche-swept and crevassed, and thin labyrinths of yawning gulfs and crumbling precipices, offer dangers which only the most powerful motives could compel one to dare.

[2] These old stranded pot holes are often found a hundred feet above the river levels.

Looking southward along the range from the summit of Mt. Humphrey, the peaks seem to be sown broadcast over a wide area. Scores of them exceed thirteen thousand, many are fourteen thousand feet high, yet so closely are they compacted that few rise more than two thousand feet above their own proper bases. Even this degree of individuality is seldom attained, for they are still further connected by the jagged walls that swoop across the interspaces from shoulder to shoulder in magnificent curves. To give effectiveness to any one of them, those adjacent would require to be removed. However awe-inspiring and incomprehensible these vast mountain assemblies seem to be, their common origin and the facts of their individual history are problems easily solved by the patient student.

Beginning with pinnacles which are the smallest of the summit mountainets. No geologist will claim that each pinnacle was formed by special upheaval, nor that the little chasms which separate them were made by subsidence, or the rending of the rock, for many of them are scarcely a foot deep, and may be formed artificially wherever the structure is suitable, simply by the removal of a block or two that has been loosened by the development of cleavage planes.

We therefore conclude that pinnacles were formed by the removal of the materials which once filled the intervening chasms.

The Sierra Pinnacles are from one to nearly a thousand feet in height, their forms and dimensions being determined not by cataclysmic fissures but by orderly joints and cleavage planes, which give rise to leaning forms where the divisional planes are inclined, as on the crest of Mt. Ritter, or vertical as at the head of the south fork of King's River.

The Minarets to the south of Mt. Ritter are a mass of partially separated pinnacles about seven hundred feet in height, forming a portion of the main axis of the range. Glaciers are still grinding their bases upon the eastern side; those of the west have succumbed to climate with one feeble exception.

In like manner the formation of the small peaklets, which roughen the flanks of the larger peaks, can be shown to be simply the results of orderly erosion and transportation of material.

The same truth applies to the larger peaks; for, when we examine the contiguous sides, we find the same structure is continued

across the wide valleys and névé amphitheatres, showing that there has been a removal of the material that once filled them.

The presence of particular veins often makes this portion of the argument exceedingly conclusive, as in two peaks of the Lyell group where quartz veins are continued from one to another across the wide valley. Just as we find rows of pinnacles set upon a base, the cleavage planes of which do not admit of the development of pinnacle forms, so do we find immense mountains like Dana and Gibbs resting on a smooth granite pavement.

They are all residual masses of the once solid wave of the entire range, and all that would be required to obliterate them would be the restoration of the materials which have been carried away.

What has become of this material? Not the millionth part of it is now to be seen. It has not been engulfed, for here are the solid unfissured bottoms. Portions of it we find scattered far and near in moraines; it is easily recognized by peculiarities of mineralogical composition. That *Glaciers* were the transporting agents, and that, in forming the basins and valleys in which they flowed, *they carved out the summit peaks*, is so important a proposition as to demand careful attention to its proof.

The ice-created valleys of the Sierra are of all magnitudes from the minute striæ of the polished surfaces the hundredth of an inch in depth, to Yosemites half a mile or more. No one questions the origin of the striæ, nor that the grooves or next larger depressions are due to erosion.

The same proof applies to the valleys of greatest magnitude; to obliterate them we have only to restore the complements of broken columns and strata, the stumps and edges of which appear on the bottoms and sides.

I have shown in previous papers (published in the "Overland Monthly") that Yosemites are only emphasized portions of ordinary valleys, and that they invariably occur where there has been a concentration of glacial force. *Their trends* are direct resultants of the forces of the ancient glaciers which acted upon them, modified by obvious peculiarities of the structure of the rocks in which they are eroded, *the trend always changing according to the force of the confluent entering tributary.*

This truth is illustrated in all the valleys of the region. We

give a single example, the Upper Tuolumne valley, which is eight miles long, and from two to three thousand feet deep.

In tracing it from its head, on the north flank of the Lyell group, we find it, after trending steadily about two miles, making a bend of a few degrees to the left.

Searching for the cause of so immense a phenomenon as the bend of a great valley in the solid rock, we perceive on the *opposite* or right wall *a depression*, which proves to be the mouth of a tributary valley, that conducts to a crater-shaped ice-womb. From this womb the ancient tributary glacier emerged, which in thrusting itself into the main valley trunk caused the depression we are studying.

After maintaining the new trend, thus modified, for about a mile and a half, the huge valley swerves lightly to the *right*. Again searching for the cause, we discover another ice-grooved tributary valley entering *on the left*, which likewise conducts back to an ice-womb, whose glacier in uniting with the main valley trunk deflected it like the first, as far as its force, modified by the direction, smoothness and declivity of its channel, enabled it to do. Still lower down, the noble valley has been pushed round in a curve to the left by a series of small tributaries which, of course, entered on the right.

The Dynamics of the Ancient Glaciers are sublimely simple: with each change in direction there is always a corresponding change in depth or width, or both.

On nearing the basin of the Tuolumne Mer de Glace it is seen to be swept entirely round to the west by powerful currents which descended from the flanks of Mts. Dana, Gibbs, Ord and others. Everywhere we see the same delicate compliance to glacial force, and no other; throughout the whole period of their formation the valleys of the Sierra were lithe as serpents, and winced at the touch of every tributary.

As to their *comparative size*, *we find the greater the ice basins the greater the valleys* which proceed from them. No such direct and simple relations exist between water currents and the valleys in which they flow.

Thus the Tenaya basin *is not one-fourth the size of the South Lyell*, yet its valley *is much larger*. Indeed some valleys have no streams at all, and the topographical conditions of the locality demonstrate the impossibility of their ever having had any.

Post-glacial streams, as we have already observed, have not eroded the five hundred thousandth part of the valleys in the upper and middle regions. Neither could *pre-glacial* streams have exercised any appreciable influence upon their formation, for their trends are *strictly glacial*, and *essentially* different from water trends.

Neither can we suppose *fissures* to have exercised any primary determining influence, there being no conceivable coincidence between the trends of fissures and the *exceedingly specific trends* of *ice-currents*.

Granting, therefore, that the great mass of eroded and transported material removed from between the summit peaks was the work of glaciers, it remains to be shown what force or forces have sharpened the crests which bear no trace of glacial action, and which were probably always above its reach. Again we find this mighty ice-tool *undermining*, from beneath, what it could not grind away; from above, eroding backwards into the bases of the peaks and pinnacles, and thus increasing the effects of winter snow avalanches that sweep their sides.

Notwithstanding the ruinous, crumbling aspects of the summit peaks, they have suffered very little change since the close of the glacial period, for, if denudation had been more extensive, their separating cirques and gorges would be more encumbered with débris; we find only a mere sprinkling of detritus, and that very little material has been removed by *post-glacial streams* is proved by the fact that the small lake basins through which they flow are not yet filled up.

We will briefly consider the formation of a few special mountain peaks, as an illustration of the foregoing principles.

The Lyell Group lies due east from the Yosemite valley at a distance of about sixteen miles in a straight course. Large tributaries of the Merced, Tuolumne, Rush and San Joaquin Rivers, take their rise amid its ice and snow. Its geographical importance is augmented by its having been the centre of dispersal for some of the most influential of the ancient glaciers.

Lyell, the dominating mountain of the group, reaches an altitude of thirteen thousand two hundred feet, though it is only a few feet higher than adjacent peaks, and all that individually pertains to it is a small fragment less than a thousand feet high, the existence of which is owing to slight advantages of structure and

position, which prevented its being eroded as fast as the common mass of the range.

Glacial fountains are rounded in a horizontal direction at the head for the same reason that they are at the bottom, this being the form which offers greatest resistance to glacial erosion.

A *semicircular outline* is thus formed, which the glaciers maintain in pushing their way back into the mountain against which they head.

Where these curved basins have been pushed quite through the axis of the chain or spurs, separate mountains or peaks are produced, the degree of whose individuality depends upon the extent and variation of the erosion.

It may excite surprise that the snow collected in these fountain basins should pass through the névé condition so suddenly and begin to erode at the very head; that this is the case is shown by unmistakable traces of such erosion upon the bottoms and sides of wombs now empty. The change of climate which broke up the glacial period would naturally cause an earlier transformation of snow with eroding ice and produce the present condition of things as a necessary consequence.

The effects of *shadows* in prolonging and intensifying the action of portions of glaciers are shown in moraines, lake basins, and in the difference in form and sculpture between the north and south sides of valleys and mountains, facts which I cannot stop here to present. It is only to their effects in the *segregation* of mountain masses that I would direct particular attention. An examination of the excellent detailed map of the Sierra, adjacent to Yosemite, made under the auspices of the Geological Survey, will show the direction of extension of the glacial amphitheatres—*they* are in a southerly direction, beneath the protection of shadows cast by peaks and ridges.

Again, it will be seen that because the Merced spur trends in a southerly direction, its western slopes are in shadow in the forenoon, its eastern in the afternoon; consequently it has a series of glacial amphitheatres *on both sides*, while another ridge trends in an easterly direction and its southerly slopes are scarcely at all in shadow, consequently all its glacial amphitheatres are on the northern slopes.

The same admirable obedience to shadows is conspicuous in all parts of the range; the few surviving glaciers are seen following them. Glaciers are the only *eroders which are thus governed.*

The Merced spur, lying about ten miles to the southeast of the Yosemite valley and almost the same distance from the main axis, presents a very instructive range of peaks eroded from a single mass. The fragments are Mt. Clark, and Red, Black, and Grey Mts. Here we have a range of peaks from eleven thousand five hundred to twelve thousand feet high, blocked out by the heads of two series of glacier tributaries.

The truly magnificent group of granite summits stretching forty miles from Mt. Humphreys in a southerly direction is the loftiest of the chain, abounding in elevations of fourteen thousand feet, yet, considered individually, mere pyramids one or two thousand feet high, crowded together and set upon a common base, like pine trees along a ridge.

Mt. Whitney lies to the south of this group, and although the very highest point of the range, geographically or scenically has no special significance; viewed from the north or south it resembles a helmet or the Scotch Cap called Glengarry. Its summit is somewhat flat, curving gently towards the Kern River valley, but holding a bold, precipitous front towards Owen's valley of some two thousand feet. These precipices gradually merge into accessible slopes around from southwest to northwest. Mts. Dana, Humphreys, Emerson and Ritter far exceed Whitney in grand impressiveness of mass and outline. A few cold meadowless lakes nestle around its base, but it possesses no glacier, and the end of summer finds little snow on its north side, and none at all on the south.

Towards the close of the glacial epoch, the summit of what is now called Mt. Whitney peered a few hundred feet above a zone of névé, which fed glaciers that descend towards Owen's valley on the east, and Kern on the west. The former cut the eastern face of the mountain, the latter its southern, western and northern flanks, conferring upon the mountain its special individuality.

Instead, therefore, of its being a grand uplift, it is merely a portion of the common mountain mass which, from relative conditions of structure and position has suffered a little less degradation. The same is true of all the mountains under consideration. The difference between the peaks and the passes is not that one are elevations and the other depressions; *both are depressions* differing only in degree, the abasement of the one having been effected at a slower rate are left behind as elevations.

All the mountains of the flanks have been subjected to the long continued denudation of the ice sheet which flowed above them.

The transition from spiky, angular down-flowed summit peaks to these overswept forms is exceedingly well marked. All weak towers, pinnacles and crumbling angular surfaces, at once disappear, leaving only knotty domes and ridge waves as illustrations on the grandest scale of the survival of the strongest. Because the ice sheet was universal there remains upon all the wide flanks of the Sierra not one weak mountain form. Near the upper limits of overflowed mountains there are no exceptions to this rule even in appearance. It is only where we descend to regions longer left bare by the ice, and therefore more weathered that we find instances of mountain rocks *at once weak in structure and strong in form.*

There is now a rock near Yosemite which is ready to fall apart by the development of its E. 35° W. cleavage planes, yet it is certain that it once stood the strain of Yosemite Creek glacier.

If the Sierra were built up of one species of rock homogeneous in structure, then we might have difficulty in finding data upon which to ground our estimates of the quantity of denudation accomplished by the ice sheet and separate glaciers in the development of the flank mountains.

Some portion of the main axis as well as outlying spurs are capped with slates, and all the base is slate-covered. The circumstances connected with their occurrence in these localities furnished proof little short of demonstration that they once covered all the range; and from their known thickness where they occur, we may approximately estimate the quantity removed where they are less abundant, or wanting.

Furthermore where the granite comes in contact with the slates, and for a considerable depth beneath the line of contact, it partakes in some degree of the physical structure of slates, enabling us to determine the fact that slates have covered the granite where none are now visible, and at all depths Sierra granite has a structure which enables us to know whether or not it has been broken.

In the restoration, therefore, of the range to its pre-glacial condition, the valleys would first be filled, secondly all the granite domes and ridges would be buried until the surface reached the level of the base of contact with the slates.

Finally, the missing slates would be restored which would bring the range upward.

From the above data we conclude that the average thickness of the restored rocks in the mid granite region would not be less than a mile.

This immense quantity of denudation it will at once be perceived, has given ample scope for the formation of the loftiest mountain domes by the removal of the weaker adjacent rocks.

As we descend from the summits on the path of the ice currents noting where they have successively denuded the various rocks, first the slates, then the slaty granites, then curved granites, we detect a constant growth of specialization, and ascent into higher forms. Angular masses cut by cleavage planes are comprehended in flowing curves. These become more highly organized and combined, giving rise to the magnificent dome scenery for which the Sierra is unrivalled.

Had the unsculptured mass of the range been everywhere homogeneous, glacial denudation would still have produced summit mountains not differing essentially from those we now find, but the rich profusion of mountains and mountainets so marvellously individualized would never have had existence, the whole flank would have been planed down in barren uniformity.

We can easily understand that an ice-sheet, hundreds or thousands of feet in thickness, slipping heavily down the flank of a mountain chain will wear its surface unequally according to the varying hardness of its rocks, but this is not the only element productive of inequalities, the glacier does not only wear rocks, as a tool wears the stone upon which it is whetted, it also breaks them up into blocks from a few inches to forty or fifty feet in diameter, the size being determined by their hardness and cleavage, and the quantity and direction of application of glacial energy upon them.

All the granite I have seen is cut by three series of separating planes, two nearly vertical, the other horizontal, which, when fully developed, divide the rock into parallelopipeds.

We measured the direction of the strike of hundreds of the two vertical series, many of which run unbrokenly for miles in a tolerably uniform course, those of the better developed series at right angles to the trend of the range, the others parallel with it, both cutting to a depth of at least five thousand feet, as shown in cañon sections.

In some localities they are extended within a few inches of each

other, while in others the edge of only one clearly marked plane is visible in a polished pavement half a mile square.

Again, large domes occur that exhibit none of these planes, appearing to be entirely homogeneous in structure. The question therefore arises:—Are these domed masses cleavageless, or do they really possess the same cleavage as the surrounding rock in an undeveloped or latent condition? Careful observation proves the latter proposition to be true, for upon the moist surfaces of some of the older domes that have been severely weathered, we may detect incipient planes running parallel with the others. The spray of waterfalls never fails to develop these divisional planes no matter how structureless the rock may appear.

Other (series of) cutting planes pass *diagonally* through those we have been considering, giving rise to pyramidal and roof shaped forms. Diagonal cleavage attains fullest development in the metamorphic slates. To it, under certain conditions, are due the huge gables so often found facing deep cañons, as at the south side of Tuolumne cañon, and the "Three Brothers" in Yosemite.

Curved structure also prevails at certain depths in the Sierra Granite, giving rise to domes, cones, rounded wave ridges, and an innumerable brood of modified forms.

We have seen that dome or curved structure offers no absolute interruption to the passage of the planes of angular structure.

We also find that the concentric layers of domes cut one another. The "bricks," thirty feet high, resting directly upon a smooth well curved dome surface, which rests in turn upon a still larger curved surface, demonstrate that neither curved nor angular structure results from *folding or stratification*.

The Yosemite half dome, the noblest rock of the Sierra, perhaps, also furnishes admirable illustrations of all kinds of cleavage and of their effects upon form. Its upper part, for about one thousand eight hundred feet, is almost absolutely vertical, while the lower three thousand feet is inclined at an angle with the horizon of about 37°. On the notch a section of the dome may be seen, showing that it is there made up of immense slabs set on edge, evidently produced by cleavage planes, which, cutting the dome perpendicularly, have determined the plane of its face.

Along the base of the vertical portion the stumps of slabs are exhibited, which have been successively split off the face while

the rock was in process of formation by the Lyell, Tenaya and Hoffman glaciers; residual fragments of the same slabs may be seen. At the summit we see the cut edges of concentric layers which have determined the curved outline. A small gable also appears, produced by the development of diagonal cleavage planes, cut in front by vertical planes. On a fragmentary dome, situated on the south side of the Mono Trail, near the base of Mt. Hoffman, remnants of concentric shells, from five to ten feet thick, are seen on the up-current side. Sections of unremoved shells are visible. This is an admirable illustration of the mode in which a wide and deep ice current clasps and denudes a dome, removing one after another of its concentric shells while it still maintains its own general form. Tracing the striæ we learn that it has been eroded at once on the front, back and sides. Where the pressure has been greatest, the erosion has been least because the layers have been pressed against each other, instead of away from one another, as on the sides and back, and could not, therefore, be so easily broken up.

The lack of individuality so observable among the summit mountains is chiefly due to the comparatively uniform structure and erodibility of the rocks out of which they were developed.

Their *forms* consequently are in a great degree dependent upon the glaciers which have wrought them But the strongly structured and specialized mountains and mountainets *of the flanks*, while accepting the ice currents as developers, still defended themselves from their destructive and form-bestowing effects. In general, the stronger and more specialized the structure of any mountain, the greater will be its ability to control the destinies of its own shape.

In a profounder analysis of the causes of mountain forms we shall come to see that those we have been considering are only proximate, and that neither ice, water nor any other eroding agent has done much more than to *disinter* their buried beauty. Some other force or forces, crystallization perhaps, *moulded* them while they yet lay buried in the common mass of the range. In general *the grain* of mountains determines their surface forms; the grain of a mountain oak is not more gnarled and interwaved than is the Sierra granite.

The ice-sheet is the only eroder which works with reference to the grain of mountains on a large scale.

The wonderful adaptability of ice for the development of buried mountains will be more and more understood as it is studied.

Granite crystallizes into landscapes, and snow crystallizes above them to bring their beauty to the light. Here it smooths a pavement by slipping flatly over it and *removing* its inequalities like a common plane. Again it *brings out these inequalities*, gliding over and around the clusters of domes, feeling out every weak spot, sparing the strong, and destroying the feeble—irresistible, yet compliant like the wind.

Rocks are brought into horizontal relief on the sides of valleys wherever superior strength of structure or advantageous position admits of such development, just as they are elsewhere in a vertical direction.

Some of these are of a magnitude that well deserves the name of *horizontal mountains*. Magnificent examples of them are seen in the profound cañons of the Merced, Tuolumne and King's Rivers.

That the variability of resistance of the rocks themselves accounts for the variety of those horizontal features, is shown by the prevalence of this law. *Where the uniformity of glacial pressure has not been disturbed by the entrance of a tributary, we find that where valleys are narrowest the rocks are strongest.*

In the case of valleys with sloping walls, its salient features are developed in an oblique direction, but neither horizontal nor oblique mountains or mountainets can ever reach as great dimensions as the vertical, because where a retreating curve is formed in a weaker portion of a valley wall, its surface receives less and less pressure, while the salient curves receive more, the farther they project into the past sweeping glacier, thus tending to limit the irregularity.

So intense is the pressure brought to bear upon some of those salient bosses, they are sometimes broken off on the lower side with a fracture like that produced in blasting. These fractures are the highest expressions of glacial energy I have met.

The same tendency toward maintaining evenness of surface obtains to some extent at least, in the case of vertical erosion, as where a hard bar extends across the path of a glacier, or an isolated rock rises out of a comparatively level area exposed to the full sweep of the overpowering current.

If vertical cleavage be developed in such rocks, moutonnéed

forms will result, *with split faces turned away from the direction of the flow*. These split faces are exceedingly abundant in the granite region, from a few inches to over a thousand feet in height. If no cleavage planes are developed, *long ovals* will be formed, whose greatest diameters extend parallel with the denuding current.

In vertical erosion the general tendency is to make the valleys deeper and ridges relatively higher. The ice current being constantly attracted to the valleys, causing erosion to go on at an accelerated rate, and withdrawn from the more resisting ridges, until finally they emerge from the ice-sheet altogether.

Thus it appears that all the mountains of the range between lat. 36°, 30′ and 39°, whether the lofty Alps of the summit, the richly sculptured dome clusters of the flanks, or the burnished bosses and mountainets projecting from the sides of valleys—all owe their development to the ice-sheet of the great winter, which brooded them all, and flowed grandly above them like a wind.

In all these sublime chapters of Sierra history there has been no upbuilding, but a gradual dismantling, and of this, all its elevations and depressions are the records and monuments.

DARLINGTONIA CALIFORNICA, AN INSECTIVOROUS PLANT. By WM. M. CANBY, of Wilmington, Del.

THE Natural Order of Plants Sarraceniaceæ is composed of but three genera and eight species. The six species of Sarracenia are found native along the Atlantic slope of the United States and principally south of Virginia, one species only extending northward to Newfoundland and thence westward to Michigan. The single species of Heliamphora[1] has been found only on a mountain of British Guiana or Venezuela; while the remaining member of the family, the *Darlingtonia Californica* of Torrey,[2] inhabits a few

[1] Bentham in Trans. Linn. Soc. Vol. 18, p. 423, et seq.

[2] "On the *Darlingtonia Californica*, a new pitcher plant from Northern California. By John Torrey, F. L. S."—Smithsonian Contributions, April, 1853.

bogs at an elevation,[3] of 6000 to 7000 feet in the northern mountains of California beneath the snows of Mt. Shasta. The Sarraceniæ, having their home in a country long since brought under the dominion of civilized man, have been subject to the observations of naturalists for many years. Yet with perhaps a single exception, the *Sarracenia variolaris*, the peculiar adaptations and structural arrangements which so admirably serve their purpose as insect catchers have not been thoroughly studied. Indeed it was not until Dr. Mellichamp's most interesting and instructive observations on the above-mentioned species were made that its operations were fully understood; though Dr. Gray has shown us that many of the facts connected therewith had long ago been made public by Dr. McBride and Mr. Elliott.

Having been much interested in researches of this character, and fully cognizant of Dr. Mellichamp's experiments while they were being carried on, it was very natural that I should turn to the nearly related Californian plant, in order to ascertain if possible if it had similar insect-preying habits. It was first discovered in 1842 by Dr. Brackenridge of the Wilkes' Exploring Expedition.[4] But his specimens were too scanty and imperfect to warrant a description, and it was not until 1851 that the late Dr. Torrey received additional ones in a flowering state. Still two years elapsed before his excellent description and plate were published by the Smithsonian Institution. Even then ripe seeds had not been obtained, and some years still passed before Dr. Gray's description of them completed the systematic diagnosis.

A plant so rare, known only as inhabiting a few stations in a rugged, almost uninhabited and little visited region, while thus affording sufficient material for systematic description, has as yet given but slender opportunity for observation upon its physiological structure and habits. For while good seeds have been obtained and distributed, the plant has not yet, so far as I know, been cultivated with even moderate success. I have therefore had to rely upon an examination of dried specimens, and the descriptions of a single observer of the perfect living plant for an investigation of its insect-catching properties, and what is here offered is not so much to give a thorough description of its structure and func-

[3] *Fide* Lemmon in litt. Prof. Brewer says he has found it at an elevation of only 1000 feet.

[4] *Vide* Torrey *supra*.

tions, as to draw attention to it, in order that fuller knowledge of a very rare and curious plant may be obtained.

All the species mentioned in this paper are "pitcher-plants," so called from the very peculiar structure of the leaves; that part corresponding to the petiole or stem of the leaf being a more or less elongated and dilated tube, in growth erect or somewhat reclining, and capable of holding a considerable quantity of fluid. In Sarracenia this tubular portion is open at the summit, and is surmounted by a hood which corresponds to the usual lamina or blade of the leaf. This hood is erect in some species, leaving the orifice of the tube more or less exposed, while in others, as in *S. variolaris*, it is curved completely over the open tube, so much so, indeed, as to prevent rain from falling or even being blown into it. In Darlingtonia the structure of these organs is essentially different. Here we have indeed the elongated, nearly erect, but *twisted* tube; but its summit is vaulted and expanded, and so bent over in one direction that the orifice is brought directly underneath and covered over completely by it. It is therefore utterly impossible — the leaves being in their natural position — that any rain or bog-water could get into the tubes. Their summits are curiously mottled with green veins and semi-transparent yellowish spots in much the same manner as *Sarracenia variolaris*. The orifice in the largest leaves is a little over an inch in diameter. At the outermost point of this opening, an organ corresponding to the hood in Sarracenia but of a very different and peculiar shape is found, being narrow at its insertion but widening rapidly, and soon bifurcating into two divergent laminæ, the whole having a striking resemblance in shape to the tail of a fish. This appendage points downwards, — the extremities apparently curving somewhat inwards. It is somewhat mottled, the main color varying from the ordinary green of the other parts to a deep brown or red. The inner side has many short stiff bristles pointing towards the orifice. So far as I have been able to make out from the dried plant the upper edges of these laminæ are rolled inwards for nearly their whole length, to an extent ranging from $\frac{1}{16}$ to $\frac{1}{4}$ of an inch, gradually widening from the outer extremities to the aperture of the tube, each thus forming a sort of groove enlarging towards, and leading directly into, the orifice. Here they join a similar turned-in process or fold which extends all around the inside edge of the orifice. The last however is wider, stouter and

more rolled in than the first. In this connection another organ is to be noticed; it is the wing more or less developed in all the species of this order, extending from the orifice of the tube to the ground. In the plant under notice it is narrow, perhaps never much over a quarter of an inch in width, and appears in fact to be two wings united for the greater part of their length, but separating within the orifice, where they form the folds already described, and also near the ground, where they become dilated and membranaceous, and assume their proper position on each side, of what is there an ordinary petiole springing from the rhizoma; so that in a morphological aspect, the whole structure is a singular modification of an ordinary winged petiole bearing at its extremity a deeply emarginate leaf! So far as the Sarraceniæ are concerned, much the same structure of the wing is discernible; and the view above taken receives additional confirmation from the structure of Heliamphora, the "pitchers" of which have each two separate yet contiguous wings running side by side from near the base to the orifice.[5]

Having given this brief sketch of the structure it now becomes necessary to show how it may be used for the trapping of, and preying upon, insects. From lack of information and experiment this cannot be done as yet very satisfactorily. In my inquiries on this point I have relied very much upon the evidence of Mr. J. G. Lemmon, who, living in Northern California, has, on several occasions, had an opportunity of seeing the plant in its native haunts, and who has most obligingly communicated recent specimens and given me all the information he could. That it is an insect-trapping plant to as great an extent as other and better known members of its family there can be no doubt. The dried

[5] Since this article was written I have had an opportunity to examine some leaves of *Sarracenia Psittacina* Michx. In their structure we may notice a near approach to those of Darlingtonia, the upper part of the pitcher being almost as in that plant. There is the same ventricose expanded summit which, if the leaves were erect, would bring the orifice underneath, and the fold within the orifice is not only present but proportionally many times larger. To be sure the "fishtail" appendage of Darlingtonia is not found here. But that admirable arrangement for attracting *flying* insects is not necessary in this plant, which has its leaves reclining in a rosulate cluster in such fashion, that the orifices are in a vertical instead of a horizontal position; thus presenting an open door for ambulatory insects, easy of entrance but extremely difficult of exit. As a consequence the prey corresponds to the structure, being composed principally of ants, with a proportion of the smaller spiders, beetles, etc. The hunter or fisher has often occasion to construct and use traps made on modifications of the same principle; but the arrangements of their mechanisms are poor and inefficient compared with those of these humble plants.

leaves received have in them, often to the height of several inches, the remains of captured insects. In letters received from Mr. Lemmon he says, "The plant I assure you is a flytrap of the most successful kind. The petioles are often thirty inches high, inflated and growing larger at the apex, where they swell into a thin bladdery transparent hood, projecting out over the wing of the petiole, and pierced by a round orifice beneath, and the true leaf pendent like a swallow's tail from the outer edge of hood. Within this hood is secreted a saccharine fluid, which is very attractive to insects. The inner side of the inflated petiole is clothed with long stiff hairs pointing downwards. Several inches of the bottom of the tube are filled with a clear fluid (secreted by the leaves it must be), and I have always found any leaf of age to contain a large quantity of insects, or their remains in it. While bringing home plants in my buggy to see if I could cultivate them, the "Jack Hornets" crowded into them so that I had often to slit the leaves with a knife, or turn them over, to let those escape that were above the water." Mr. Lemmon has kindly sent me an ounce phial completely filled with the fluid "from two petioles." Furthermore there is some evidence in the last number of the "Bulletin of the Torrey Botanical Club," that in one locality the leaves are employed as flytraps, just as those of *Sarracenia variolaris* have been. It is scarcely necessary to say, that as it is certain no water can get into the tube by any ordinary means, and as the fluid is always present in healthy leaves, it must be secreted by the plant as Mr. Lemmon says. I have quoted above what he says of a sweet secretion, exuded in the vaulted part of the tube, which is very attractive to insects. It is fair to say that in a recent visit to the growing plants he did not find it, but he adds that he "remembers distinctly that on former occasions it was quite apparent." I have no doubt that the plant has such a secretion which is used as a lure or bait in the same manner as in its allies the Sarraceniæ: for in some leaves long ago collected by Miss N. J. Davis, and which had been stuffed with cotton while in the fresh state, the cotton though easily removed from that part of the tube below the level of the aperture (where, it should be stated, for some distance there are neither bristles nor saccharine secretion) adhered with some tenacity to the upper part, and even to a portion of the appendage outside. The extent of this exudation is very plainly to be seen in many of the dried leaves which I have examined.

More to the point however than this is the fact that while the bristles of the vaulted part may not be secretive, the whole surface is sprinkled with minute glands which are doubtless organs of secretion. There is also some evidence of a secretion extending along the wing to the ground, though the weight of the testimony is against it. Mr. Lemmon thinks he has observed no instance of this. But it is fair to say that he has had no opportunity for observation since my letter containing specific inquiries on this and other points was received. There are along the edge of the wing, or rather along the hollow caused by the two wings not being completely united at their edges, minute brown glandular bristles, and sparingly intermixed, lighter-colored, nearly circular bodies which may be glands; one or both perhaps furnish a sparing secretion. Against this is the strong testimony of Prof. Riley, to whom the insects found in the leaves were submitted, that he finds no ambulatory ones among them, but that they are all such fliers as would be likely to be attracted towards, or arrested in, their course by the appendage.

Here then we seem to have a bait "that is very attractive to insects" leading directly to "a trap of the most successful kind." Mr. Lemmon further says, "I came upon a patch once in September and smelled it from afar so offensive was it. A portion of the leaves filled with insects to the depth of four to six inches, had fallen down apparently from the weight of the fluid and in sects."

But let us consider the probable uses of other organs. Dr. Mellichamp having shown the presence and use of the secretion on the edge of the wing in *Sarracenia variolaris*, a similar arrangement in Darlingtonia would infer a like use. But even if this be not verified, insects flying near the plants could hardly fail to be attracted by the peculiar bright colored appendages. Alighting upon either lamina of an appendage they would sooner or later crawl upwards, and would be inevitably directed towards the orifice by the rolls or grooves already described, aided by the thick set upward pointing bristles. Once persuaded inside by either route or attracted by the "honey pastures" (as Dr. Mellichamp well names those parts covered by the sweet secretions), there would be little chance of their escaping. For they would not be likely to fly downwards and so out of the orifice, while from but a small portion of the inside surface could they *drop* out; be-

cause if their hold was loosened while upon the sides of the vault over the orifice, the fold or rolled up portion upon the inside edge of it would retain them within, and I believe would also be sufficient to prevent them from attempting to crawl out. Here too would come in play the semi-transparent spots, if Dr. Mellichamp's ingenious surmise be correct, that by admitting light in another direction than from the orifice, they serve to attract and baffle the insect in his endeavor to escape. It is evident that these "areolæ," being successively in the line of the sun's rays, must admit more light and be more brilliant than the opening below; just as numerous windows in a roof would light an uppermost room very much more than a hatchway in the floor. It is to be noted too, that these light-admitting spaces extend some distance down the back of the tube and away from the orifice, and this I believe will be found to have its effect in luring the insects to destruction. As a further preventive of escape we find the inside of the vault clothed with innumerable bristles pointing backwards and downwards, as is the case with all the plants of the order. Thus we see how an insect might be attracted to the appendage, led along by the outside grooves to the orifice aided by the minute bristles which all point towards it, attracted inside from this or from the wing by the sweet secretions, prevented from falling or crawling out by the inside folds, baffled in any endeavor to escape by the bright areolæ and the retrorse bristles, and finally led to the ultimate and inevitable well of death. Surely there is enough in all this, to say nothing of other points yet unnoted, to tempt those who have the opportunity to make accurate observations of the functions and operations of this wonderful plant.

One other and a very curious characteristic of the leaves remains to be noticed. They are all twisted upon their axes about one-half a turn. So far as I have been able to observe, all the leaves of a plant twist in one direction. But leaves of different plants may be twisted in opposite directions, and if my specimens are any criterion, there are about as many winding from right to left as from left to right. I have not been able to divine the purpose of this twisting; but that it does subserve some useful purpose in the economy of the plant I have no doubt. Observation of the growing plant will probably furnish a solution.

Finally, I wish briefly to call attention to the floral organs. The flower is solitary and nodding upon a scape, which Mr. Lemmon

says is generally about one-third higher than the leaves. Beneath the five sepals and the five petals, the twelve to fifteen stamens are clustered around the uppermost and thinnest part of the almost exactly bell-shaped ovary. Under this and corresponding to the clapper of the bell, is found the five-parted style, with its divisions recurved and stigmatose at the extremity, and therefore pointing somewhat upwards under the ovary. Such an arrangement renders it as impossible for the granular pollen of the flower to drop upon its stigmas, as it would be for a marble dropped upon a bell to strike the clapper. Consequently the flowers are not self-fertilizing. Knowing that the flowers of all the species of Sarracenia must from their peculiar formation be cross-fertilized by the aid of insects, it is evident that this plant will be found to be another instance of the same fact; and it would not be at all wonderful if the only remaining plant of the order, the Heliamphora, should be found to correspond with its allies in this respect.

Having thus described, as well as I was able, with the means at hand, some of the functions of this most curious plant, I hope to draw such attention to it that its wonderful operations may soon be fully elucidated.

ORIGIN OF THE CASCADES AND OF THE SUBMERGED FOREST ON THE COLUMBIA RIVER, OREGON.[1] By WILLIAM P. BLAKE, of New Haven, Conn.

IT is generally known that the Columbia River has cut its way through the basaltic rocks of the Cascade Range, leaving cliffs on each side from 2500 to 3000 feet high, but it is not so generally understood that the stream at that point flows at a higher level than it formerly did owing to a partial filling up of the channel. The evidence that the river once flowed for a considerable period of time at a lower level is found in the submerged forests of fir trees extending for twenty-five miles or more along the valley

[1] The observations upon which this paper is based were made in the year 1867.

above the rapids. The trees stand erect as they grew, but the tops above the water have decayed and broken off, leaving only those portions that have been protected by the water. The wood is not petrified, but is much softened on the outside, though the heart remains sound. These trees were evidently firs identical in species with the firs now covering the mountains around. It is thus certain that the submergence occurred at a comparatively modern or recent period. The Indians have traditions that Chinook canoes from the mouth of the Columbia formerly passed readily up the stream as far as a high fall at the Dalles, some fifty miles or more above the cascades. They say that there was a bridge of rocks across the river, but that during a "fight," as they term it, between Mt. Hood and Mt. St. Helens, this bridge fell in and the waterfall at the Dalles was at the same time so reduced in height that the salmon were able to pass above it, and the Indians of the upper country were no longer obliged to travel to the Dalles for their supply. The current of the river at the cascades is now so broken by a series of rapids that it is no longer navigable.

The steamboats from below stop at the lower part of the cañon and transfer their freight and passengers to a railway around the rapids. The obstruction which has raised the bed of the river and formed these rapids is due to the sliding in of enormous masses of the adjoining banks. This is the correct and generally accepted explanation by those familiar with the country, and is also presented by Dr. Newberry in the Pacific Railroad report, but the *cause* of this sliding in has not yet been stated. The substratum of the basaltic beds is a hard volcanic conglomerate, resting directly upon soft sandy and clayey beds, containing trunks of trees and fossil leaves. The edges of these soft strata, may be examined at low water at the base of the bank opposite the lower landing. The strata and other interesting features of the Cascade Range have been well described by Professor Joseph LeConte in a recent number of the American Journal of Science.[2] Besides these beds so exposed there is another, of blue unctuous clay, visible in the stream only during low water, which is gradually squeezed outwards and upwards by the enormous weight of the mountain resting upon it, and as it exudes is washed away by the current. It is upon

[2] "On the great Lava Flood of the West, and on the Structure and Age of the Cascade Mountains." Am. Jour. Sci., vii, No. 39, March, 1874.

this soft and plastic stratum, or similar beds that portions of the mountain appear to move slowly and may be said to be *launched* into the channel. The land-slides thus do not appear to result from direct undercutting of the cliffs by the stream, but to be due to the unstable plastic foundation, the movement of which is permitted by partial uncovering, while it affords a plane of little friction upon which mountain masses may slide. At the Lower Cascades, in 1866, the right bank of the river was believed, upon good evidence, to be in constant though slow motion towards the channel; so, also, on the left bank about half-way up and above that point. The point of land crossed by the railway slid downwards several feet in the course of four years. The railway where laid in a straight line became curved, and had to be relaid. There is thus evidence of lateral movement of considerable masses of the mountains at the Cascades, explaining satisfactorily the great accumulation of rocks in the stream by which the channel has been partially filled up and the waters set back over the forests of the valley.

We thus see how it is possible there may be considerable lateral movement in mountain masses, when built up as the Cascade Range is upon an unstable foundation. I do not, however, regard the whole range as resting upon plastic sediments. A large part of the principal axis is supposed to be made up of older rocks. The lateral extent of the stratified substratum, at the Cascades, was a problem which required patient exploration to solve, and the time at my command did not permit of it. Prof. LeConte's explorations of three of the principal creeks prove that it underlies the range at that part extensively, and that it may properly be regarded as the foundation upon which the immense bodies of lava were poured out.

A Remarkable Ancient Stone Fort in Clarke County, Indiana. By E. T. Cox, of Indianapolis, Indiana.

Of the many ancient works scattered over the greater portion of the United States, indeed, I might add over North and South America, there are none which possess a greater interest for the student of archæology than the stone and earthwork enclosures,

built by the race of men who inhabited the country at a time so remote in the past that the tradition of the races of men who succeeded them cannot furnish us with any trustworthy clew to their history. Though they were evidently a race living under organized governments, at the head of which there was, probably, a powerful and despotic ruler whose will was law, yet amidst all the remaining records of their greatness no trace has been found of a national name, consequently they are classed under the general name of *mound-builders.*

Without relating the many evidences which lead to the inference that the mound-builders of the United States had their origin in Central America, where civilization and arts attained a degree of perfection unsurpassed, if equalled, by its earliest traces in the Old World, I will merely add that in their emigration to the northward, they followed the shore lines of the oceans and gulfs and penetrated to the interior by the great rivers and their tributaries, and took possession of such river sites for their towns and forts as our forefathers subsequently saw fit to adopt as sites for founding some of our present flourishing cities.

One of the most remarkable works left by the mound-builders is seen in Clarke County, Indiana, fourteen miles above the Falls of the Ohio River. It is an ancient stone fort, built upon the terminal point of a high ridge, which is washed on the south side by the Ohio River, and on the north by Fourteen-mile creek, a broad and deep stream which empties into the Ohio a short distance below.

The point of the ridge is pear-shaped and the fortification includes from eight to ten acres. The highest point at the stem of the pear is 280 feet above the Ohio River and is here only ten to twenty feet broad, with the corniferous limestone two to three feet thick exposed at the surface, and the Niagara limestone beneath, and reaching down in an almost perpendicular wall to the river. A natural wall of Niagara limestone also furnishes complete protection against the approach of an enemy at the upper part of the fort with the exception of a short gap on the creek side, extending from the upper point southward for about 100 steps. This break in the natural wall is protected by an artificial wall, seventy-five feet in height, made by laying up loose stone, mason fashion, but without mortar. The base of this wall for sixty-five feet in height follows the slope of the hill and then rises ten feet vertically.

Around the southern terminus of the point there is another artificial stone wall ten feet high which connects the two natural walls of Niagara limestone, thus forming a complete barrier to the approach of foes. On the inside of the made wall are numerous mounds of earth, and within the line of these mounds there is a dug ditch four feet deep and twenty feet wide which receives the drainage from the crown of the hill.

A trench was dug through one of these mounds which revealed slabs of stone set upon edge and leaning in the direction of the slope of the mound. They extended inward for two feet and were arranged so as to protect the mound from the overflow of the ditch.

Some pieces of charcoal, ashes, decomposed bones, and a large diamond shaped rock having a slight indentation near the centre of one surface, and worn quite smooth, are all the relics we could find in it.

On top of the ridge there are also a number of mounds, and the ground slopes very gradually from the northern to the southern extremity the former being 280 and the latter 160 feet above the river.

Physical History of New Hampshire. By C. H. Hitchcock, of Hanover, N. H.

ABSTRACT.

The physical history of New Hampshire signifies a sketch of the growth of its territory from the earliest times when it was an archipelago of islands down to the present moment. The subject is a novel one, nobody having studied the history of our country in that way heretofore. The region is the theatre of conflicting theories among geologists. I accept the theory of the very great antiquity of the rocks of the State, but do not discuss the reasons for it at present.

The first great period was characterized by the formation of porphyritic granite, the largest ranges of which occupied the watershed between the Connecticut and Merrimac rivers. The crystals

of felspar in it are very coarse and usually arranged without order. There were twenty islands of this rock at the close of the first great period, the most northern lying at the south edge of the White Mountains. This period is supposed to have been the equivalent of the Laurentian.

Next there is a series of gneisses for which the name of "Atlantic Group" was proposed nearly forty years ago by Featherstonhaugh. I gave the name of White Mountain series to this formation in 1869, and retain that name now in a restricted sense. The Atlantic series consists of three divisions. These gneisses fill up the spaces between the several islands of porphyritic gneiss. The Mount Washington range of mountains is composed of the most recent of these, and seems to have been elevated at this time.

The next great period is termed the Labrador or Labradorian period. It is the most peculiar of all that are here considered, and I can find no descriptions in any geological work of a similar structure. The rocks occupy the southwestern area of the White Mountains, and are composed of granites and compact felspars. There seems to have been a hydrographic basin, with Mount Washington upon the east, Lafayette upon the west, Black Mountain in Sandwich upon the south. This basin was filled with an overflow of granite to the depth of 1,000 feet and about 400 square miles. After the consolidation of this liquid granite, it was covered by another flow of different mineral character. The first was a coarse reddish granite, the second was deficient in quartz. Some portions of this area were covered by a third eruption of granite, best seen on Mount Chocorua. These granites, in turn, were covered by thick deposits of clay which are altered into compact felspars, both Labradorite and orthoclase. These several layers are now essentially horizontal, but streams have worn out valleys in the midst of the plateau and left many isolated mountains.

The topographical features are to be compared with those of Western Virginia, where deep ravines have been excavated out of a horizontal plateau. This structure has never before been observed, because the country is entirely away from roads, and covered with the primeval forest. The State next received accessions along the Connecticut River valley, particularly in the upper portions. The formation is termed Huronian, and is of very great thickness.

The next period is the most extensive of any. The rocks con-

sist of varieties of *mica schist*, which is abundant along the Connecticut valley and the sea-shore. The formation covers unconformably rocks of the Atlantic period. The last period exhibited on the map shows the area submerged by the ocean in the Helderberg period. It occupied principally the Connecticut valley to the depth of 1,000 feet.

The paper was illustrated by a series of large maps, which cannot be reproduced in this report on account of the expense.

On Wet Processes of Copper Extraction. By T. Sterry Hunt, of Boston, Mass.

ABSTRACT.

Copper is generally got from its ores by smelting in furnaces; but for centuries it has been known that by acids it can be dissolved from its oxidized or calcined ores and from the solution thus obtained thrown down by iron in the metallic state. A cheaper mode of obtaining its solution was sought, and by roasting the sulphurous ores with sea-salt it was found that the copper could be got as a soluble chloride. This process is largely used and offers great advantages, especially for ores holding but little copper. But the consumption of metallic iron in precipitating is large; three tons or more of salt are wasted in getting each ton of copper, and moreover arsenic and other impurities find their way into the products. The speaker then described a new and more economical process devised by himself and Mr. James Douglas of Quebec, which is based upon some little known chemical reactions of copper and iron, described to the Association at a former meeting. When oxide of copper is brought in contact with protochloride of iron, the latter is decomposed, the iron being thrown down as peroxide, and the copper changed into a mixture of one-third of soluble protochloride and two-thirds of protochloride, insoluble in water, but soluble in strong, hot brine. From this solution metallic iron throws down all the copper as

metal, and regenerates the protochloride of iron, ready for use again. This series of changes may go on indefinitely.

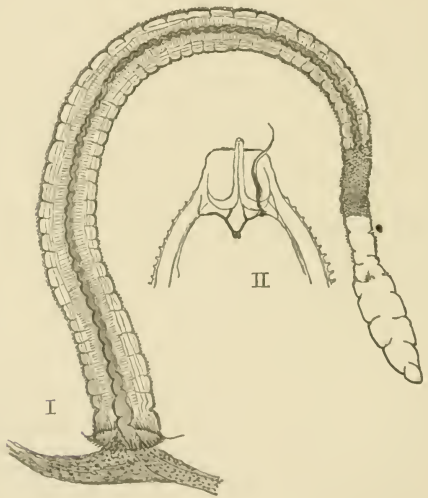
To prepare sulphurous copper ores for this treatment it is only necessary to calcine them at a low red heat. The injurious elements of the ore, such as arsenic, antimony and tin, remain undissolved in this process, and the copper is obtained so pure that it can be refined by a single fusion. If, however, silver be present it is dissolved, and may be readily separated, so that this process is adapted to silver-bearing copper ores like many of those in the west.

The speaker described the works recently erected at the Ore Knob mine in North Carolina, where from 3000 to 4000 pounds of copper are made daily by this process at a much lower cost than by any other method. Furnaces and tanks to increase largely this production are there already in process of erection. Photographs of the present works, and specimens of the copper in a beautifully crystallized form, were exhibited, after which the author briefly noticed the copper deposits abounding along the Appalachian belt from Newfoundland to Georgia. He had previously called the attention of the Association to the importance of those deposits in the Blue Ridge as sources both of copper and sulphur, some of which may yet be used in treating the phosphates of South Carolina in the manufacture of fertilizers. At an early day the copper of that region bids fair to become an article of export.

The copper ores of our mesozoic or new red sandstone formation were then alluded to. In many localities these rocks hold an abundance of the metal so disseminated as to be unavailable for any smelting process, though fitted for treatment by some wet method like that just noticed. He had recently visited the copper deposits of this formation in Chester county, Pennsylvania, and described their mode of occurrence and their geological significance. Similar deposits are known in the mesozoic of the Connecticut valley, and deserve more attention than they have yet received.

On an Organ of Special Sense in the Lamellibranchiate Genus Yoldia. By William K. Brooks, of Cleveland, Ohio.

The chief peculiarity of the lamellibranchs as a class is the great disparity shown in the development of the organs of vegetative and animal life; while the digestive and circulatory systems are very highly evolved, the nervous system and organs of special sense are characterized by their extreme simplicity. Besides the feeble sensibility which seems to be shared by all external parts of the body, the siphonal openings are furnished with fringes of



very simple tentacles, sufficiently sensitive to warn the animal of the presence of foreign bodies; the organs of vision, so widely distributed among all classes of animals, are here very simple, and are little more than "eye-specks," along the border of the mantle of certain species; the ear is represented by a still more simple sac, embedded among the soft muscles of the foot, and must yield very feeble sensations of sound. These are all the sense-organs hitherto described in this class, and their simple structure and lim-

ited power and efficacy are what we should expect from the sedentary life of the animals, their more or less perfect protection from enemies, and their method of obtaining their food from the water which bathes their bodies. It is therefore interesting to find, in the genus Yoldia, which exhibits remarkable locomotive power, a very perfect and highly specialized organ of feeling adapted to the examination of objects at some distance from the body.

About half-way between the central line of the siphon and the lower margin of the mantle, on the right side, there is a retractile tentacle, capable of protrusion to a distance equal to the length of the shell, and movable in every direction with great accuracy and rapidity. When the animal is at rest with the siphon tubes extended, the tentacle may be seen, projecting from the open shell, and lying loosely coiled upon the bottom of the aquarium. Occasionally it may be seen extended to its full length, moving in all directions, and examining neighboring objects as if in search of food, and it undoubtedly performs this function, as the siphon tubes may be seen to follow its motions as if directed by it. The siphon is not simply a channel through which water flows by ciliary action, but a true respiratory organ, contracting rhythmically, and expelling the water by muscular action. The lower or incurrent siphon is not a closed tube but it is open along the lower median line, and at the base, each side is folded back and fastened to the sides of the mantle; occasionally the tentacle is retracted, and thrust between these folds and then projected through the incurrent tube to a distance of half an inch or more beyond its tip, and quickly withdrawn. This motion is so rapid that very careful observation is necessary to detect it, and it is repeated at short but irregular intervals, and seems to be for the purpose of examining foreign bodies which may have gained access to the tube and lodged there; for the respiratory motion of the siphon usually ceases at the time the action is performed, and occasionally the siphon is retracted also. The tentacle has also been seen to examine foreign bodies which have adhered to the outside of the siphon.

The tentacle, when fully extended, is very delicate and thread-like, but very much convoluted and wrinkled when retracted; it is covered with a single layer of epithelial cells, which adhere so slightly that it is difficult to remove the organ for examination without destroying them. Beneath the epithelium is a layer of longitudinal muscular fibres; and inside this, a layer of circular

fibres, forming a tube which contains a large nerve fibre surrounded by a delicate sheath; when the tentacle is retracted the nerve is thrown into convolutions within the ring of muscle, and its foldings are therefore independent of those of the outer muscular wall. At the base of the tentacle the nerve enters a ganglionic mass, situated upon the nerve which passes backward from the parieto-splanchnic ganglion to the siphon and mantle, and is between the origin of the siphonal branch and the point of division of the nerve trunk to supply the margin of the mantle.

The activity displayed in the use of this tentacle; the complication of its structure, and the great development of its nervous connections; and the fact that it is developed upon only one side of the body,—all indicate a very high specialization, and even if it should be shown to be homologous with any of the sense organs known in this class, its great development seems to separate it from them very widely.

Its structure seems to be somewhat similar to that of the tentacles at the tip of the siphon, but its nervous connections are entirely different. Its position is somewhat similar to that of the "eye-specks," but its organization prevents any such comparison, and we seem justified in ranking it as a new organ of special sense.

AN INQUIRY RESPECTING THE REVERSION OF "THOROUGHBRED" DOMESTIC ANIMALS. By WM. H. BREWER, of New Haven, Conn.

THE foundation of our agriculture rests upon our domestic animals, and in the more modern times the improvement in our live stock has been much greater than the improvement in our tillage.

The two great elements in this improvement in our domestic animals are, *first*, the wise selection of breeding animals, and, *second*, their care.

Both of these are essential. If animals be starved they will not thrive no matter how well bred, and on the other hand, if ill bred, no amount of care or feeding will give us the best beef or milk from our neat cattle, the best wool and mutton from our sheep, nor the

highest speed or other good qualities in our horses. These facts are so well known and admitted that we may call them axioms.

I need not try to prove (what all will admit) that in our most celebrated breeds, their special excellences are the accumulated improvement of many successive generations, and that this improvement is often accompanied with marked and important changes of form and structure. Now, as this improvement and change are unquestionably the results of man's care, it is often stated that if the care be withdrawn, then the breed will retrace its steps, and go down by the same road that it came up (only vastly faster, the downhill road being the easier travelled), and become again what it sprang from, or to use the more familiar phrases which are supposed to sound more scientific, it "*reverts*" or "*returns to the original type.*" This has been said so often that it is believed, and with a great class of scientific men has become a dogma, and as such is used to sustain certain theories regarding the permanence of "original types," and the fleeting nature of "acquired characters." As a dogma, it finds a place in the lectures of eminent scientific men, and also in our scientific literature, and in the papers read before our learned societies, and from these high authorities it spreads and descends through various channels until at last it is proclaimed by the popular lecturer or finds a lodgment in the "Agricultural Column" of religious weeklies.

The claim of high scientific authority gives it peculiar weight with a great class of farmers, who venerate science, but who do not represent the most advanced and enterprising of their vocation, who are cautious, who do not wish to expend the requisite care on their cattle to enable them to have the very best, and who content themselves with stock of more moderate excellence. Knowing that thoroughbreds are costly property, they think it "fancy stock," holding its excellence and value by a very uncertain tenure, they seem to them a precarious or speculative investment, liable to "revert" on their hands. The knowledge that such cattle will *deteriorate* indefinitely under bad management, combined with a belief (backed by such high authority), that there is moreover a special law tending to make them "revert" to some definite poorer form, is one great cause impeding the general improvement of live stock.

At the last meeting of this Association, this dogma of reversion was strongly insisted on in one of the papers, and I find on p. B,

404, of the last volume of its "Proceedings," this,—"The hog has been greatly changed by domestication, and yet, when left to himself he soon returns to the original type. During the late war *some of the most improved breeds* were turned loose and left to shift for themselves. *Three years after, I found them possessing all the physical characters of the wild boar of Europe.*" I have *italicized* in the quotation the special point to which I wish to call attention. At the reading of this paper the author verbally stated that a similar fact had been observed with "Durham Cattle." About the same time the same dogma was strongly and publicly affirmed by another scientific authority who is even more eminent. In both cases the dogma was used as an argument to sustain a certain scientific hypothesis, or else to combat another hypothesis.

Now let us turn to another meeting, held scarcely a month after our last one. A herd of short-horns was to be sold at New York Mills, and stock-breeders flocked from near and far to attend this other meeting. They came from England, they came from California, they came from half the states of the Union; they even came from the very regions where these scientific authorities had but just told them that short-horns were so prone to return to their original type. There was less talk at that meeting than at the scientific one; the speeches were in fact *very* short, but they were to the point. The principal speaker was the auctioneer. He was doubtless eloquent in his way, but his eloquence alone will hardly explain the result, for in thirty minutes from the time he began, the sales amounted to a quarter of a million of dollars, and in a very short time, those reckless men, unheeding the warnings of their scientific brethren, bought one hundred and nine head of cattle for $382,000, an average of more than $3,500 per head. For the best five cows (belonging to a particularly excellent strain called the "Duchess") they paid on an average $31,640 per head. *These prices indicate the faith of those men in the future permanence of the acquired characters of that herd.*

For some years I have been looking for the proof of the oft-repeated assertion, that thoroughbred stock of old and well established breeds ever returns to the original type (whatever that may mean), *under any conditions or circumstances.* Thus far the results have been entirely negative, that is, I often hear the general assertion, but I have not yet found the proof in specific cases. Until within the past year, however, my inquiries have been verbal,

as I had opportunity, and I kept no record of the answers received, because, I thought that among practical men (so called) the thing was settled. But the confident assertions within a year, by such eminent authorities, that *such reversions would always take place under certain conditions*, and then, following so closely on the heels of these assertions, the remarkable sale of which I have spoken, have awakened a new interest in the subject, and I have resumed the inquiry, in the hope that, if there be any such great law, somewhere we may find the proof in specific examples.

I have therefore prepared the circular which follows, and I ask of the members any aid they may be able to give. The questions have already been presented to quite a number of breeders. I find it easier and pleasanter to do it by printed circular than orally, because, when with innocent simplicity I have asked some of these haughty breeders (who pay as much for a bleating calf as I get for two years salary), if their short-horns ever reverted into something else, I have thought I have sometimes detected a smile which I feared indicated a want of that veneration which is due the members of this learned Association, in their pursuit of knowledge under difficulties.

I append the circular I have sent, and I purpose to give this Association the further results of the inquiry at some future meeting.

"*Dear Sir:*—Will you have the kindness and patience to answer me the following questions:

1st. Have you, personally, ever known any case where *thoroughbred* short-horn cattle, because of climate, poor feed, neglect, or any other cause, have become in character anything else than short-horns,—in other words, *where from any cause thoroughbred short-horns have degenerated into animals of any other breed or type?*

2d. Do you personally know of *thoroughbred* animals of any other breed so changing or "reverting?"

3d. Have you ever heard of such a thing taking place in the experience of other breeders, so well authenticated that you believed it to be a fact?

Now please let me explain why I trespass on your time and good nature with questions that may seem to you to be simply absurd.

I think that the practical breeders of thoroughbred stock (of

whatever kind) commonly believe that so long as the breed is kept pure and no other blood mingled, that although the animals may vary greatly in excellence, all of them will have the essential characters which distinguish that breed from all other breeds or "types."

On the other hand, many persons (who are not breeders of thoroughbred stock so far as I know) have asserted that if neglected, any breed will "revert to the original type," or by other words or phrases have carried the idea that, under such or other circumstances, the breed will not only lose its better qualities, but, moreover, that the animals will become of some *other breed or type* which is said to be that of the early and cruder ancestors from which the breed originally sprung, however long ago that may be. Within a year this assertion has been again strongly put forth by several men, eminent in other departments of knowledge, one of whom (a Professor in a Western University) has stated that such reversions were common, were in fact the rule in the "West." If this be true, then the practical value of thorough breeding must be much less than is usually claimed.

Formerly as a farmer, and later as Professor of Agriculture in this school, I have for many years sought specific and authentic examples of such "reversion," but thus far have never found a single case. The statement, however, is so often found in print, and is so confidently put forth by scientific men of more or less repute, that I am again trying to learn more definitely what is the actual experience of breeders of thoroughbred stock whose skill and reputation give weight to their testimony, should it please them to answer the questions proposed. The answers may be short if explicit.

That *grade* animals often "revert," that curious freaks and "sports" often attend violent crossing (and also that breeds *deteriorate* under bad management or bad conditions), are well enough known, but these facts do not affect the specific questions asked where *the blood is supposed to be kept strictly pure.*

The questions are asked in the interest of both science and practical stock-breeding, and answers will be gratefully received by

Yours, respectfully,

WM. H. BREWER.

Prof. of Agriculture in the Sheffield Scientific School of Yale College, New Haven, Conn.

July, 1874.

REGENERATION, OR THE PRESERVATION OF ORGANIC MOLECULES: A CONTRIBUTION TO THE DOCTRINE OF EVOLUTION. By LOUIS ELSBERG, of New York.

IT may be confidently asserted that most naturalists are nowadays inclined to admit the truth of the general doctrine of Evolution, the difference of opinion among them relating principally to the method in which evolution has been brought about.

As to the fact of evolution, so much discussion has already taken place, that I have no desire of evoking any more before the Association. So far as I can judge, all the lines of observation and of reasoning lead to the same conclusion. Rejecting it is equivalent to abandoning the attempt to explain the constitution of the organic world. "The genetic relationship of organic forms is as certain as the law of conservation of force and matter in the inorganic kingdom; in fact it is nothing else than the application of this most general law to living nature, and implies nothing else than that all material existence is subject to the same conditions." (Nägeli.)

As to the factors of evolution there is still need of thorough and individualizing investigation. It is well known, Lamarck "attributed something to the direct action of the physical conditions of life, something to the crossing of already existing forms, and much to use and disuse,—that is, to the effects of habit." (Darwin's "Origin of Species," 5th edition, 1871, Historical Sketch.) Heinrich Baumgärtner in 1853 (Lehrbuch der Physiologie), endeavored to explain Evolution by so-called germ-metamorphosis, periodic transmutations of the embryo in its primitive stages,—something similar to which Kölliker seems to mean by what he terms (Ueber die Darwin'sche Schöpfungstheorie, 1864,) heterogeneous generation. In 1858, Darwin and Wallace called attention to the important factor natural selection, which had, however, been previously indicated by Dr. W. C. Wells in a paper read before the Royal Society in 1813. Darwin added sexual selection ("Origin of Species," London, 1859, and "Descent of Man" 1871), and his latest utterance is that it has been effected "chiefly through the natural selection of numerous successive slight favorable variations, aided in an important manner by the inherited effects of the use and disuse of parts, and in an unimportant manner, that is, in relation to adaptive structures, whether past or

present, by the direct action of external conditions, and by variations which seem to us, in our ignorance, to arise spontaneously." ("Origin of Species," 6th edition, 1873, p. 421.) Recently the effect of local isolation has been sought to be determined (Weissmann, Einfluss der Isolirung auf die Artbildung, Leipzig, 1872). More recently still, Nägeli has studied the societary or cœnobitic origin of new species (Sitzungsbericht der mathematisch-physikalischen Classe der k. b. Akademie der Wissenschaften zu München, February 1, 1873). The chorology or geographical and topographical distribution of both animals and plants, the survival of the fittest and supplanting of the less suitable in the struggle for life, the effects of crossing, the laws of heredity, the circumstances of individual development, those of individual variability, both sudden and gradual, and the accumulation of variation in a series of generations, all have been discriminatively investigated by able inquirers, and much light has been shed upon the factors of evolution within the last few years. Nevertheless, we are very far from being able to say that either the efficient external conditions of the transmutation of one species into another, the environs of the organisms, or the internal causes, those residing in the constitution of the organisms themselves, are fully understood.

Again, no question, perhaps, in physiology, has occupied the attention of thinking persons more than the mystery of generation. Two hundred years ago Drelingcourt, a celebrated physician, collected no less than 262 different hypotheses on the subject from the writings of earlier authors, to which he added his own. Since his time this number has been largely increased; yet none has been found satisfactory, the mystery still remains impenetrable. The wonder expressed by Montaigne in 1580 ("Quel monstre est ce, que cette goutte de semence, de quoy nous sommes produicts, porte en soy les impressions, non de la forme corporelle seulement, mais des pensements et des inclinations de nos pères? cette goutte d'eau, où loge elle ce nombre infiny de formes? et comme porte elle ses resemblances, d'un progrez si temeraire et si desreglé, que l'arriere-fils respondra à son bisayeul, le nepveu à l'oncle?" Essays de Michel de Montaigne) is reiterated by Louis Agassiz in 1873 ("when we analyze it in all its bearings, we shall see that there is matter enough for wonder, and that we as yet know almost nothing about the mysterious problem of life. What can there be of a material nature transmitted through these

bodies called eggs, themselves composed of the simplest material elements and arising in the female organism without coöperation of the male; what influence can there be, I repeat, by which all peculiarities of ancestry belonging to either sex are brought down from generation to generation?" Lecture IV, delivered before the Museum of Comparative Zoölogy of Cambridge, Mass., March 27, 1873), and Hæckel in 1874 ("we reflect with wonderment on the undeniable fact that the simple ovicell and the single semen-filament or spermcell so accurately transmit the individual vital molecular movement of these two individuals, that afterwards the minutest physical and mental peculiarities of the parents reappear in the child." Natürliche Schöpfungsgeschichte, 5th Edition, Lect. VIII).

All naturalists know how difficult it is to distinguish the human embryo from that of any quadruped; indeed, neither chemical nor microscopical analysis discloses the slightest difference between the germs of not only different individuals but of different species and groups, which, nevertheless, in their development are widely different. The explanation of the phenomena of reproduction, which I beg leave to bring to your notice, is intended to show wherein the difference consists.

To simplify our inquiry as much as possible, let us in the first place ask: wherein do the germs of two different human beings differ? Obviously, if two different women are impregnated by two different men, the germ of the child of each couple differs from that of the other child, in the very fact that each is composed of the particular ovum and semen derived from its own parents. Not having considered this too obvious for statement, let me go on: The impregnated ovum, from which a child proceeds, consists of matter wholly derived from the bodies of its parents; in the same manner, the germ from which each of these parents has sprung, existed in the bodies of their parents. This cannot be gainsaid; what I assume is that some of the parental matter is retained in the child's body to go to the formation of the new being. As the child's whole body has grown from the germ originally derived from the bodies of its parents, it is certainly not unreasonable to assume that the ovum or semen, as the case may be, which the child, later in life contributes to the formation of a new being, contains a few of the identical material particles which constituted the germ from which it arose; in other words, that the germ of

this new being contains actual particles, plastid-molecules or plastidules,[1] of its grandparents.

To those who can more readily conceive the idea of force being transmitted than matter, it may make the subject clearer to state that I regard a "plastidule" quite as much a centre or bundle of force as of matter. Nor is the specific transmission of the identical plastidule the point necessarily insisted upon, since the transmission of particles rendered identical by assimilation and growth meets the purposes of the argument, as presently stated. Nevertheless actual material transmission appears to me the most probable.

With the assumption of transmitted plastidules, the fact of the resemblances in features of children to grandparents, other in-

[1] Hæckel has given the name Plastid, meaning forming or formative matter, to the units or ultimate form-elements to which organic bodies are reducible. In man and the higher animals and plants these are, as is well known, the simple minute structures called cells, but in lower beings they are even less complex than cells and are called cytodes. The difference between cells and cytodes is that the latter consist only of apparently homogeneous and structureless albumenoid, jelly-like substance, while the former contain within similar jelly-like substance a differentiated nodule called nucleus, and sometimes even a still more minute nodule, a nucleolus, within the nucleus.

Schleiden, who was the first who announced, as to plants in 1838, the most important generalization for which biology is indebted to the microscope, viz., that every organic body is made up of either a cell or a congeries of cells and their products, defined a cell to be: "the elementary organ which, completely developed, possesses a wall of cellular substance and semi-liquid nitrogenous contents, and which forms the only essential form element of all plants, without which no plant exists."

Schwann (Mikroskopische Untersuchungen über die Uebereinstimmung in der Structur und dem Wachsthum der Thiere und Pflanzen, Berlin, 1839) who extended the cell theory to animals by demonstrating that the animal organism as well as the vegetable, is composed of cells and cell derivatives as ultimate form elements, laid greater stress than Schleiden upon the nucleus, so that while according to Schleiden the cell essentially is a vesicle consisting of a closed sac or membrane and semi-liquid contents, according to Schwann it consists of cell membrane, contents and nucleus. As it was found that most animal and vegetable cells possessed a nucleus, the three constituents as laid down by Schwann came to be considered the requisites for the definition of a cell. But further investigation, especially of animal cells, showed the membrane or cell wall frequently absent; so that Leydig (Lehrbuch der Histologie des Menschen und der Thiere, 1857) could state that "not all cells are vesicles, not always can a membrane be distinguished separable from the contents." The term cell-contents was therefore changed to cell-substance, and in 1861 Max Schultze ("Ueber Muskelkörperchen und das, was man eine Zelle zu nennen habe," Reichert and Du Bois-Reymond's Archiv, 1861, p. 1) defined a cell to be "a lump of protoplasm containing a nucleus." This definition has been adopted by most histologists; hence, while we may speak of cells with or without cell wall, we cannot properly speak of nucleated and unnucleated cells, a nucleus being essential to the constitution of a cell. The low form elements devoid of a nucleus were in 1866 by Hæckel (Generelle Morphologie der Organismen 1866, vol. 1, p. 270) called *cytodes* (cell like) to distinguish them from *cytes* or cells.

The cell-substance, formerly called cell-contents, has received various names, such as *sarcode* by Dujardin, *germinal matter* by Beale (in contradistinction to the cell wall

heritances good and evil, predisposition to disease, atavism, etc., are quite naturally explained. The objection might be urged that although the germs, and for some time after birth the bodies, of the parents had contained plastidules of the grandparents, yet the metamorphosis of tissue that had gone on in the parental bodies previous to procreation, must have brought about a complete change of every particle, and, therefore, destruction and removal from the body, of the grand-parental plastidules; but this objection is of only apparent significance:—for, aside from the idea already stated as being sufficient for our argument, viz., that nutritive assimilation or growth impresses new molecules with all the qualities of the old, and thus preserves the identity, we must consider that the material of the body breaks down and is eliminated only through use, and that the use through which the

or other products of cell life, which he calls *formed material*), *cytoplasm* by Kölliker, and *protoplasm* by Mohl. The latter name has been almost universally adopted to designate not only the cell-substance but also the cytode-substance. But, as Van Beneden ("Recherches sur l' Evolution des Grégarines" Bulletins de l' Académie royale de Belg. 2nd series, vol. xxxi, 1871) properly says, a distinction should be made between the substance which makes up the cytode and that differentiated substance which together with the nucleus makes up the cell. In recognition of the fact pointed out by Hæckel, that *plasma* etymologically means the "formed" and that the "forming" should be termed *plasson*, Van Beneden applied the name plasson to the undifferentiated substance of cytodes, leaving the word protoplasm undisturbed to designate the cell-substance. I propose that Beale's distinction of germinal matter and formed matter be recognized by the terms plasson and plasm, and that the germinal matter or forming substance of cytodes be called, neither protoplasm nor simply plasson, but protoplasson, and the cell-substance, not protoplasm, but cytoplasson. The *formed* material of cytodes, such as investing membrane, etc., must then be called protoplasm, and the formed material of cells cytoplasm.

The form-elements or plastids either have an investing membrane or are nude or naked, as it is called; there are, therefore, four different kinds of plastids, viz.: (1) Gymnocytodes, or nude cytodes, *i.e.* unnucleated plastids without investing membrane; (2) Lepocytodes, or covered cytodes, *i.e.* unnucleated plastids with investing membrane; (3) Gymnocytes, or nude cells, *i.e.* nucleated plastids without investing membrane; and (4) Lepocytes, or covered cells, *i.e.* nucleated plastids with investing membrane.

The smallest partition-particles, or molecules, of a plastid I have called *plastidules*; which I define to be the smallest particles in which the qualities of a plastid reside: the really "elementary factors of plastid life," as Hæckel ("Zur Morphologie der Infusorien" Jenaische Zeitschrift für Medicin und Naturwissenschaft, vol. vii, No. 4. 1873, p. 22) expresses it. A plastidule as I conceive it, is a plastid-molecule; but this is not exactly synonymous with protoplasm-molecule, as Hæckel (loc. cit.) takes me to mean. In the case of a cytode, plastidules are "the hypothetical sarcode-particles which constitute the properly elementary factors of plastid life," but in the case of a cell, the plastidules or properly elementary factors of plastid life are more than the subdivisions of the cell-substance (the cytoplasson, or protoplasm of previous writers) as distinguished from the nucleus. Although the precise function of the nucleus is still unknown, it probably plays an important rôle in the generative or multiplying process, and as I conceive the plastidule to be the effective elementary factor in this very process, it must necessarily be held to include, in the present state of our knowledge, the subdivision of the nucleus as well as that of the cell-substance.

materials under consideration are removed from the body is only the generative act,—so that their conservation until used for generation may well be assumed. The assumption made, and we shall not find it difficult to believe that the germ of a child contains molecules or plastidules or bundles of force derived through its parents from grandparents, great-grandparents, and in fact from a long line of ancestors. The difference, then, between the germs of two different persons, not recognizable by chemical reaction or microscopical inspection, but ascertained by another mode of analysis, viz.: logical reasoning, is an evolutional one, and consists in the fact that each contains the molecules of its own line of ancestry.

The further back we trace the genealogy of a child, the more we begin to understand the indescribable complexity of the genetic constitution of its germ. Who can tell how many generations have passed between the child of to-day and its progenitors in the far past, or the first human parents from which it has sprung? The question whether all men living have come from one original ancestral pair, or several, is irrelevant to our present inquiry; but let us commence with a primitive "Adam and Eve." Their children came from germs which were wholly derived from their bodies; the germs of the children of these children contained, mixed with the modified plastidules of their immediate progenitors, some of the original plastidules of the first parental pair; and so on in each succeeding generation. The further removed an ancestor is the smaller is, of course, the quantity of his share in the constitution of the germ of the progeny. To express the idea arithmetically: in each succeeding generation, the numerator remaining the same, the denominator of the fraction of the set of plastidules from a particular ancestor increases, while at the same time the number of *sets* of ancestral plastidules increases.

As, in the course of time, the subdivision of the original mass of plastidules, and the accumulation of plastidules from successive progenitors, contained in the germ, are continually increasing, the plastidules must be exceedingly small. This accounts for the inability of the chemical scale and the microscope to give us any account of them, or of the difference in composition of different germs. The necessary smallness of the assumed plastidules can, however, be used as an objection to the belief in their efficiency only by those who are unacquainted with the assumptions of mod-

ern science regarding molecules, to which I shall refer presently. I need hardly add that if the microscope or chemical means could prove the difference in composition which I urge, I would not call my views of regeneration an hypothesis. Besides I could cite many instances in which particles of matter are inconceivably small, and still exercise appreciable effects. Mine is by no means the only case in which the intellect — call it imagination if you like — can safely follow paths in physical science from which sensual perception is excluded; and when carefully guarded by reason, this use of the imagination yields results as reliable, I think, as microscopical and chemical observation and experiment.

My hypothesis of the propagation of organisms may be stated as follows: The germ of every derivative living being contains plastidules of its whole ancestry. I call it the regeneration hypothesis because according to it the ancestors are to a certain extent bodily, and therefore also in every other respect, born again in their progeny; or the hypothesis of the preservation of organic molecules because it assumes that certain plastidules are, though not forever, yet for a long time, preserved, and transmitted from generation to generation; or I might call it the hypothesis of the conservation of organic forces which, with the explanation already made, would express the same thing.

Now, plastidules are like molecules in contradistinction to atoms. The plastid-molecule, or plastidule, although inconceivably small, and incapable of division without destruction, or decomposition into its chemical elements, has actual dimensions; and the statement of the assumption that a germ contains plastidules of the *whole* line of ancestral organisms, needs, therefore, the qualification that after a certain vast number of generations, the beginning of the line must become cut off by the exhaustion of the plastidules of the most distant ancestor. This accounts in part for what has been called the law of abbreviated heredity. Other circumstances, of which we are as yet ignorant, may cause the exhaustion, diminution or lessened influence of any particular plastidules and thus cause gaps in the line. It must be, of course, also borne in mind that the transmission of ancestral plastidules takes place in each generation through material derived from the immediate parents only. The hypothesis of regeneration, or preservation of organic molecules, is in accord with all the ascertained facts of development, and explains in an unconstrained and natural manner many of these facts which have hitherto appeared

inexplicable. Indeed, it seems to me to throw light upon the whole doctrine of evolution, and to facilitate its acceptance by both the lay and scientific public.

The hypothesis has to do with derivative living beings only; nevertheless I must refer briefly to the vexed question of archigony, the so-called "spontaneous generation," or the origin of primary organisms. Of the conversion of inorganic elements into living matter, I have said in another place (viz. London Monthly Microscopical Journal, 1872, p. 183) "No one who considers the subject seriously doubts that at some past time in the history of our globe such conversion has taken place, the difference among men being that some ascribe it to the exercise of direct creative agency in each individual case, while others ascribe it in the first instance to the agency of properties inherent in matter, and to 'evolution into higher forms' subsequently; but, I repeat, the fact of such conversion must be admitted by every one, whether he be the most ultra free thinker; whether he be the most literal interpreter of the Bible which says of man, 'And the Lord God formed man of the dust of the ground, and breathed into his nostrils the breath of life, and man became a living soul;' or whether he be anything between these two extremes."

It is certain that *if* all the requisite conditions be present the conversion of inorganic elements into living matter must necessarily take place. For my own part I confess, that, while I do not know what exactly these conditions are, and while I admit that human agency has never succeeded in bringing them demonstrably about, I see no difficulty in believing that they may occur in the laboratory of nature at any time, at the present time as well as in a long past age; and that, *whenever they occur, today as well as in the remote past, the production of living protoplasson takes place.*

Let us suppose that under appropriate circumstances, no matter how, the primitive beings, the simplest protists, *moners*,[2] specks of

[2] For the first complete observations on the natural history of a moner (*Protogenes primordialis*) we are indebted to Hæckel. They date from the year 1864. In 1865 Cienkowski described a fresh-water moner (*Vampyrella*), and in 1868 Huxley the deep sea moner (*Bathybius*). There are now a number of different kinds known. Simpler living beings than minute amorphous specks without structure are not conceivable, and some of the moners living at the present day seem to be in that condition. In the case f. i. of the *protamœba* there seems to be nothing but a speck of albumenoid matter without form, without vacuole, and without structure distinguishable by any means at our command. Nevertheless, aside from movement, nutrition and growth, propagation has frequently been observed in these beings.

"protoplasson," have come into existence. How did they propagate? I answer this question, although hypothetically, yet with a certain degree of confidence, having by my own observation as well as that of others become acquainted with what appears to be the mode of propagation of *moners* now living.

At first a slight indentation becomes visible, a contraction or constriction about the middle of the speck, which gradually increasing produces something like an hour-glass shape, until the appearance becomes that of two smaller specks united together by a thread, whereupon the thread disappears and two separate specks are seen. Cienkowski, Hæckel, T. C. Hilgard and others, have also succeeded in observing occasionally conjugation of specks, although their interpretation of this phenomenon may be entirely different from mine. I conceive the division of *primitive* specks to be not "spontaneous," but the consequence of the mixture of two. Two specks of "protoplasson," no matter how perfectly alike they may be supposed to be at the time of their production, cannot remain identically similar, because their existence and surroundings in space, and, therefore, the modifying influences to which they are exposed, cannot be identically the same. If we assume that two such specks combine and are thoroughly mixed with each other, and then divide, we may draw the following two inferences: 1. The resulting specks differ from each of the original specks, inasmuch as they contain plastidules of both; and 2. The resulting specks represent accurately the constitution of the two original specks, but not as these were at the time of their production, but as they are at the time of mixture. We thus arrive at the most elementary conception of the two fundamental laws of the phenomena of all living beings, viz.: adaptation and heredity. While adaptation involves change, heredity preserves in the progeny the constitution of the parental organisms as it is at the time of mixture. Adaptation includes nutritive growth and denotes all modifications which occur in the being from the moment of its production to that of its death. The action of adaptation and heredity, after repeated generations, induces, I assume, a tendency to partition into two, four or more segments; this accounts for the occurrence of spontaneous division or self-division in the case of more advanced moners and somewhat higher organisms. The first important onward step in progressive development is the differentiation of the albumenoid matter

in the two directions, viz.: of internal nucleus on the one hand, and of external limiting layer or membrane on the other.[3] The latter seems to take place most easily, has been described by many authors as "encystation" or "encystment," and occurs sometimes just before multiplication. The former raises the morphological value of the being from that of a cytode to that of a true cell; the differentiation thus commenced becomes the starting point of the gradual change, through continual increase and physiological division of labor, to higher organisms.

The segmentation of a germ observable not only in low forms but also in the highest organisms is explained according to my hypothesis by the fact of the antecedent occurrence through innumerable generations of the mixture of different plastidules.

The limits of hybridity prove that the plastidules to be mixed, though differing, must not differ more than to a certain relatively very small degree; greater dissimilarity prevents the mixture.

It has often been asked by opponents of the doctrine of evolution why the germ of one animal is never observed to develop into another animal; and Louis Agassiz, at the conclusion of a lecture in which he had descanted upon the striking resemblances of forms in embryo, asks: "How, then, does it come that the germ of a turtle always produces a turtle, the germ of a snake always a snake, the germ of a dog always a dog, the germ of a man always a human being, if there be not something superior to these physical resemblances controlling all growth throughout the organic world?" The regeneration hypothesis answers such questions completely by showing that the germs of different organisms existing at the present day, although they may appear ever so closely related or alike, are, because they have passed through different stages of development, so completely dissimilar in their actual constitution or mixture of plastidules, that the one organism can never be developed from the germ of the other.

As is well known, the doctrine of evolution derives most important support from embryology and comparative anatomy; the regeneration hypothesis materially contributes to the explanation of the facts in both respects; and accounts also for the so-called persistence of types. As to embryology, it has been found that the successive changes of individual development correspond to the

[3] This has been regarded as leading to the two divisions of Organisms, viz., Animals and Plants.

successive introduction of organisms upon the earth as independently taught by geology. Upon this fact Hæckel's biontogenetical law is based that "the ontogeny of every organism repeats in brief time and in great general outlines the phylogeny," which means that the individual development of every organism, or the series of forms through which it passes from germ to completed form, repeats approximately the development of its race or the series of forms through which its ancestors have passed. The hypothesis shows how this is materially brought about. On examining the various modes of propagation of organisms, we find that they may be arranged in a series proceeding from the most simple to a very complex process. Hæckel has shown that they embrace:—I. Fission, including (1.) Division and (2.) Gemmation; II. Secretion, including (1.) Asexual germination or sporogonia and (2.) Sexual germination or amphigonia; with various transitional modes of propagation, linking them all closely together. In Fission a more or less fully developed portion of the parental body separates as the progeny. The difference between Division and Gemmation is, that in the former the individual organism separates into two or more coördinate portions, neither of which can be said to hold to the other the relation of parent to child, but all of which are the children of what was before an individual organism; while in the latter, buds grow out of the parental organism which are subordinate to it, of younger age and distinctly recognizable as its offspring. The buds appear either on the outside of the body as external gemmation; or, what is more rare, on the inside. Internal gemmation almost never occurs except in very low animal forms. There are cases of internal gemmation which constitute a transition from budding to the formation of spores. In Secretion a more or less entirely undeveloped portion of the parental body is the germ which becomes the child. Asexual germination is propagation by spores, which is connected with sexual germination by the so-called parthenogenesis. Sexual germination requires a mixture of two distinct materials, which are secreted either in two distinct organs of one parental organism, or in two distinct parental individuals. These two distinct parental materials are the ovum, the female portion, and the sperm, the male portion. When they are united in one individual, we have what is known as hermaphrodism; when they are distributed to two individuals we have diœcianism or, to use a term introduced by

Hæckel, gonochorism. Now, take the case of a mammal. Its immediate progenitors and its ancestors for a number of generations were mammals of the same order, genus and species as itself; but the farther back we trace its genealogy, the lower is the position in the scale of systematic zoology which its far removed ancestors held, until in our retrospection through geological æons, we come to the lowest, simplest conceivable, living beings,— moners. Reproduction, with them, was a species of overgrowth. Two or more specks joining and dividing gave rise to a generation; and the change produced in each speck by surrounding circumstances and its own action and reaction—adaptation—ensured variation in succeeding generations. All this I have before already indicated. By conjugations and divisions many times repeated, and by the individuals of each generation changing by adaptation, the progeny gradually came to contain less and less of the plastidules of the original specks, and became more and more differentiated from the primitive moners. This continual differentiation constitutes evolution, in the phylogenetic sense, which is nothing more nor less than 'regeneration' according to the laws of adaptation and heredity. When the progeny of the cytode had, after many generations, become so far differentiated from the primitive moner as to have a nucleus, it already had attained to the morphological rank which at the present day every ovum holds from which a mammal embryonically proceeds. The first animal amœba, or true cell, of course consisted of the modified plastidules of its more or less unnucleated progenitors; it contained some material particles of a moner. Generation after generation succeeded each other, and it is plain without another word, that until the mode of propagation became so far changed by adapting circumstances that secretion took the place of both division and external and internal budding, the offspring, separating from the parent by fission, had to pass through the same phases of growth as its ancestors because it contained some of their plastidules. But the same truth holds good on considering the development of the higher organisms. The offspring which instead of being an internal bud is elaborated as a germ, being composed of the plastidules of its ancestors, necessarily grows as they did; the material molecules of early progenitors make their presence felt in the changes through which the germ passes before it resembles the fully developed condition of its immediate

parents. Thus it happens that, as Agassiz says, "the echinoderms, for instance, when forming in the egg, have, first, polyp-like, then acaleph-like features, and that when they finally assume the characteristics of their class (supposing that they belong to the higher species of echinoderms), they pass through phases of the lower families before they attain their own specific features." Thus it happens that "a reptile is fish-like before it assumes a reptilian character; that birds and quadrupeds are fish and reptile-like before they develop their class features; and that even the human embryo passes through phases resembling all these lower groups."[4] Thus it happens that the embryo of every mammal presents to us a history, by means of which the history of its race may be interpreted.

As to comparative anatomy, it has been found that the various organisms now existing upon earth form a series, or, so to speak, a chain of gradations, *more or less* resembling the parallel series of forms through which higher organisms have passed phylogenetically and do pass ontogenetically or embryonically. Upon this fact is based what I have ventured to formulate as the biontographical law that "the relative rank of every organism in each large group corresponds in the main to its seniority in organic existence." Confining ourselves for illustration to the animal kingdom we find that difference of structural rank prevails throughout, both in larger and in minor groups. No one has ever considered the Protozoa as standing on a level with the Vertebrata. All naturalists agree in considering the latter as the highest and the former as the lowest type of animal life. All place the other animal sub-kingdoms between these two, and though different zoölogists differ as to the relative rank of some of them, according to different estimates of their relative structure, all acknowledge the justice of gradation accordingly as the various structures subserving the functions of the organism, respectively, are combined with, separated from, or otherwise arranged relatively to each other. Further, to quote Rolleston,[5] "Within the limits of each sub-kingdom the differentiation of organs, by the assignment of

[4] The Method of Creation. Twelve Lectures before the Museum of Comparative Zoölogy, at Cambridge, Mass. By Prof. Louis Agassiz. 1873. Lect. xii.

[5] Forms of Animal Life; being outlines of Zoölogical classification based upon anatomical investigation, and illustrated by descriptions of specimens and of figures. By George Rolleston, D.M., F.R.S., Linacre Professor of Anatomy and Physiology in the University of Oxford; Oxford, 1870, p. 19.

them to the more or less exclusive performance of particular functions, is very often carried out in the different classes to such a different extent as to allow us to speak without violence and without hesitation of such classes as being higher or lower in the scale of existence. Elevation in the scale of life is indirectly entailed in sub-kingdoms which possess air-breathing representatives, as aërial respiration renders possible a greater activity of function than an organism, differing in this, though similarly constituted in all other particulars, can put forth; whilst the special habit of parasitism, which often renders not merely single organs but even whole systems superfluous, and is then found to be correlated with the complete or nearly complete disappearance of such structures, must be regarded as entailing a true morphological degradation." On the other hand, it is true, many of the groups are coördinate, not subordinate, and cannot, therefore, be properly arranged in a lineal order. Of Protozoa, infusoria are higher than gregarinæ, amœbina and monera. Of Cœlenterata, spongiadæ are lower than anthozoa, hydrozoa and ctenophoræ. Among Vermes, the platyelminthes are much lower than the nematelminthes, bryozoa, tunicata, rotifera, gephyrea and annelida. The lowest Mollusca certainly are the brachiopoda, if they can be properly classed as Mollusks at all; Morse urges[6] that the Annelidan affinities of brachiopods are so great that they should be associated with the Vermes. Lower than the other remaining Mollusca are the lamellibranchiata, which constitute the acephala or headless mollusks; then come the Cephalophora, viz., the gasteropoda, pteropoda and cephalopoda, the latter being placed by common consent at the head of the molluscan sub-kingdom. The Echinodermata or starfishes, sea-urchins and holothurians are relatively distinguished from each other by a progressive complication of structure from the asteroidea and crinoidea to the echinoidea and holothurioidea. So with the Arthropoda, the most careless observer will notice the gradation between the water-breathing crustacea, the still incomplete though air-breathing arachnida, the more complete and air-breathing myriopoda, and the still more highly organized and air-breathing insecta. The comparative standing of different classes of the Vertebrata is so well known that I need not recall it except to say that there should

[6] On the Systematic Position of the Brachiopoda. By Edward S. Morse, Professor of Comparative Anatomy and Zoölogy in Bowdoin College, Boston, 1873.

be made two separate classes lower than those ordinarily recognized, viz., the lowest, the Acrania, formed by amphioxus, and the next higher the Monorrhina constituted by the cyclostomi. If we take any one of the other classes separately we again find gradations in it. That is, fishes, amphibians, reptiles, birds and mammals may be subdivided according to details of structure by which they stand relatively higher and lower. This series of rank, determined by relative complication of structure, specialization and differentiation, is recognized by all zoölogists, and is not a simple series, but involves endless minor series, based in the same way upon structural characteristics. And this series of rank resembles that of origin in time, *i. e.*, the successive introduction of animals upon the earth as far as geology has revealed it and as paleontology teaches. How have these resemblances and this gradation been brought about? The parallelism between the series of rank among living animals and the series of geological succession is due, as the regeneration hypothesis explains it, to *the greater or less resemblance of the influences, to which organisms, which originally were derived from more or less similar "protoplasson," have been throughout the ages past and are at present subjected;* and the gradations and differentiations among the species of living animals are due to the differences of time during which these influences have been at work upon each species. The assumption of the repeated origination of protoplasson, in all past time, since the first appearance of life upon our planet; in connection with the consideration that, as the laws of nature are immutable and uninterruptable and the same causes must produce the same effects modified by circumstances,—geology teaching that the present physical conditions of the earth, as far as they come within its domain, have been brought about by the operation, throughout the ages, of the slowly but surely acting causes which are still at work changing them, and the evolution doctrine teaching that the primary form of every organism is that of simple protoplasson — the evolution of protoplasson at one time must resemble, as far as changed circumstances may permit, the evolution of protoplasson at another: has led me to formulate into a probable phylogenetical law the idea that "the phylogeny of every organism repeats approximately (with modifications) that of organisms which have preceded it in organic existence." The degree or amount of transmutation which has taken place in an organism

after a long series of generations is a very complicated resultant embracing all the factors of evolution. The only attempt at indicating it in detail that has as yet been made concerning any organism is the representation of human descent or the genealogical tree of man published by Prof. Hæckel.[7]

In conclusion allow me to present to you connectedly though very briefly the theory of evolution consonant with the views I have expressed in this paper.

(Let me say, in parenthesis, that the theory of descent is often and by many misapprehended. No evolutionist supposes, as antagonists frequently allege, and many of them doubtless sincerely believe, that any existing form has sprung from any lower now existing form. The name descent theory carries to some minds the idea of degeneration. For such I would substitute the word ascent for descent.)

In the beginning there existed no living beings upon this planet. All living beings have been originally produced by conversion of inorganic matter first into the simplest organic being, protoplasson. This conversion or (if you choose to call it so) "special creation" has been many times repeated since the beginning and is still going on; and so has evolution, which is "regeneration" according to the laws of adaptation and heredity, been going on from the beginning and is still going on. The protoplasson which started upon its evolution first in time, is, other circumstances[8] being equal, most highly developed, which means that it may be stated as generally true that the most highly developed organisms are so, simply because they are the oldest in organic existence; those in their constitution nearest to protoplasson are the youngest. The difference of rank among existing forms is mainly due to the difference of time during which, or the number of times which, regeneration has occurred; the difference of direction in which development has taken place is in part due to the modifying or adapting influences with which the organism has been sur-

[7] Generelle Morphologie der Organismen. Berlin, 1866, vol. ii, p. 428; Natürliche Schöpfungsgeschichte, Berlin, 1870. p. 578; and especially Ueber die Entstehung und den Stammbaum des Menschengeschlechts (2nd. ed.), Berlin, 1871.

While this paper is passing through the press, Hæckel has published a work entirely devoted to human evolution, entitled: Anthropogenie. Entwickelungsgeschichte des Menschen. Leipzig, 1874.

[8] *i. e.*, possible chemical difference in different specks of primitive protoplasson, and different external conditions acting upon them either to retard or accelerate evolution, or change its direction.

rounded. Provided the distinction between 'primordial form' and 'individual primordial being' be made, I quite agree with Darwin "that probably all the organic beings which have ever lived on this earth have descended from some one primordial form."[9] I believe that all living beings higher than protoplasson have descended from one primordial form, viz., protoplasson, but from different individuals of such primordial form; these individuals have originated at different times of the earth's existence, and have possibly also more or less differed in chemical constitution. But it must not be asserted that the fossil fish of the silurian age is the ancestor of any fish now living; on the contrary, if any of its progeny have survived, it probably belongs now to a different class of vertebrates; the fish of to-day comes from protoplasson which has originated later in time than that from which the silurian fish sprang.

Man has not been most recently created, but earliest! We are really the descendants, or the ascendants, of the oldest inhabitant of our globe.

NOTES ON TREE GROWTH. By ASA GRAY, of Cambridge, Mass.

WHETHER the trunk of a tree increases in length, in the parts once formed, is still an open question in the popular mind. Last Spring I was inadvertently drawn into the discussion of it through some remarks that I offered upon a communication which the agricultural editor of THE TRIBUNE referred to me for my opinion. Finally, as the evidence adduced was more or less contradictory, I suggested a method by which the question might be settled, perhaps in a single season, viz.: by driving into the trunks of young and thrifty trees—in which such lengthening, if actually occurring, would be most manifest—vertical rows of copper tacks, at short and equal distances. This being done in Spring, before the growth of the season begins, and the intervals, say of four

[9] Origin of Species. American Edition, 1870, p. 420.

inches each, very accurately measured and marked by a sharp scratch upon the flat head of each tack, it was thought that any lengthening which took place in vigorous saplings, chosen for the purpose, would show itself appreciably in the course of the Summer, and would be indicated by an increase in the intervals upon re-measurement at the close of the growing season.

Whether my note in THE TRIBUNE inviting these experiments has been responded to, and with what result, I do not yet know. But I may report the result of my own experiments thus far; for the trees which I selected were of species that finish their annual growths before the middle of August, so that the result at the end of Autumn will probably not be different from what it is now. I made and fixed my measurements, early in April, and in the manner specified, upon very vigorous and fast-growing young trees: 1. of *Magnolia auriculata;* 2. of a Yellow Buckeye (*Æsculus flava*); 3. of a Yellow-wood (*Cladrastis tinctoria*). The girth of the trunks, at about the middle of the line of tacks, was, in the first, 6½; in the second, 12; in the 3d, 18 inches on the 10th of August. A thick layer of wood had been formed since the tacks were driven in April, especially in the Yellow-wood, which was a sucker from the roots of an older tree that had been destroyed, and in that of the Magnolia which was one of several such very thrifty suckers.

I had arranged for a similar line of measurements upon some shoots or trunks of a year old, which, to my regret, I found had not been attended to. But as to our three trunks, the short of it is, that re-measured on the 10th of August, by my steel four-inch rule, they had not lengthened at all. The four-inch distances, accurately marked in Spring, remain just four inches now.

Of course, the result is what I expected, and, if confirmed by parallel experiments which I had advised to be made, may go far to dispel what I may venture to call a popular fallacy. There is reason for believing, however, that this fallacy is on the wane. Since my proposition for testing it was published, in THE TRIBUNE, in April last, the editor has received, and referred to me, sixteen communications on the subject; an analysis of which may be worth a moment's attention. Eleven of them are to the effect that the trunks of trees do not extend in length; only two of them are clear that they do; while three opine on different grounds, that "both sides are in a measure correct." The ma-

jority of those who deny that there is any lengthening base the conclusion upon facts of their own observation. One (N. P. H.) calls attention to a row of poplars, used as posts into which cedar rails were mortised, and which stood thirty or forty years without any perceptible change in the position of the rails. He also mentions two maples, which he planted eight years ago, and at the time, after the prevalent fashion in the country, cut off the tops with his knife, at a point as high as he could reach; they have grown into robust trees, but he finds that the cut end of the original stem is still just within his reach.

G. M., of Delaware, Ohio, at the age of eighty-four years, writes a letter, in a firm hand, to say that he carved his name and the date upon the trunk of a beech tree, forty years ago, where it still remains, at the original height.

T. M., of Alleghany City, narrates an instance which recently occurred under his observation, where, in cutting down a fine and large oak, apparently intact, the clear marks of a former attempt to fell the tree were reached at a considerable depth, the notch made by the chopper many years ago remaining just at the height at which the recent chopper struck his axe.

F. E. K., Clarendon, Vt., while cutting up the butt log of a large maple tree, came upon the auger-holes of old tappings, covered by many layers of wood, the surface of which was scored by recent tappings, at about the same height; and on trunks scored with twenty or thirty tappings, of as many years, he notes that the older are not higher than the later.

H. F. D., an engineer, avers that "bench marks," made upon the trunks of trees in running railroad levels, remain quite as fixed as those upon rocks or stakes, excepting where the trees stand in wet or clayey ground, when they are apt to be slightly raised by the frost in winter.

An old surveyor, in North Carolina, testifies to a similar stability in the old notches everywhere cut upon tree-trunks as land marks, the ancient ones being at the same height as the recent ones, *i. e.*, about three feet from the ground, the height convenient to the axe-man.

Five contributors maintain the same side of the questions on theoretical grounds, or at least without special observations or experiments at first hand. I shall not stop to examine their reasons.

Of the five who think there is some truth on both sides, one (W. V. G.) might be counted with the foregoing, as he is clear that wood once formed and matured does not lengthen with the next year's growth, and he explains how, in ways that have formerly been suggested, the trunk might sometimes be raised by the reaction of the roots on a rock beneath, or a horizontal limb become higher by its own thickening and by its further blending at base with the trunk, through the joint increase of their diameters. But this contributor also calls attention to the fact, that the bark of a tree not only fissures lengthwise with the increasing diameter of the wood to which it is applied, but also to some extent horizontally, and that this indicates a vertical stretching and therefore a certain amount of lengthening of the wood of the newer layers. One can conceive this taking place in the forming layer of the season, but not how it could elevate the base of a limb joined by old wood to the parent trunk. Moreover, as the layer of the season is forming at the same time over the whole length of the tree from root to tip of branches, one does not see how an elongation is to take effect, except at the summit in the growth of the season. As to the transverse cracking of the bark on old trunks, the fact that the fissures remain so narrow as they do is good evidence that the elongation of the trunk beneath is little if any, and I should say that the transverse fissuring is no more than what was owing to shrinkage as the live bark dies, parts with its juices, and dries up. Moreover in the paper birch and white birch there is no transverse fissuring, which goes to show that their trunks do not lengthen at all.

Another correspondent (T. G. F.) who has read in the older books on structural botany a good deal more than there is any foundation for, about a collum or collar between the root and stem, and who states that the wood is here of horizontal instead of vertical fibres or cells, suggests that the trunks lengthen at their base only, and by the increase of these horizontal wood-fibres, formed one above another, at the level of the ground. Now, in the first place, this supposed collar was only between the primary root and stem of a seedling; so that only trees raised from seed could be lengthened by its growth, not those originated from suckers, layers or cuttings. Next, trunks lengthened in this way would come to have the butt log composed

of wood with horizontal fibres, which I think nobody ever saw. And finally, there is no such collar, even in the seedling, with horizontal fibre, interposed between root and stem. I am bound to record the statement of this contributor, that his theory was borne out by an experiment he made, namely, by driving year after year a small nail into the trunk of an apple tree at the level of the ground; on cutting down the tree he found the nails were one above another, instead of all on one level. Possibly the level of the ground was changing a little all this time, either by washing away or filling in.

Two correspondents only are confident that a tree trunk goes on lengthening. One of them (G. W. M.) rests the conclusion upon the following fact:

"While witnessing the workings of a drag-saw on a butt log of a large maple tree, I noticed the tapping scars were quite numerous and prominent, at least five inches from outside, and not less than eight feet from the ground. This log was being sawed into stove wood one foot in length, giving a good opportunity to see the scars or auger holes at the end of each cut up to the fifth cut. Will Prof. Gray tell us how these scars came so far up?"

I cannot tell. It is directly opposed to the observations of the Vermont contributor, mentioned above, who finds in general that ancient and recent marks of tapping are all at or near the base of the trunk, the older not higher than the more recent. It is possible that the tree mentioned by "G. W. M." may in certain years have been tapped when there was much snow on the ground.[1]

Finally, "O. H." of Hackensack brings two arguments, one from analogy, one from observation. First, he says that he gained his own six feet of stature by a gradual lengthening all through, and therefore imagines that the tree grows in a similar way. Next he refers to the long and limbless trunks of forest trees, from which five or six twelve-feet logs may be cut without limb or knot. He takes for granted that the lowest limbs of such trees, now sixty or seventy feet from the ground, were formerly nearer the ground, and have been carried upwards in the course of

[1] A gentleman present communicated, as a fact well known to him, the information that, when trees were tapped in places exposed to cattle, and which it was not convenient to fence, it was the custom to insert the spouts and hang the buckets so high above the ground as to be out of their reach.

years;—which is the very thing to be proved and is easy to disprove. For, in sawing up the clear-stuff into boards, the knots or other traces of the old limbs are to be found near the centre of the lower part of the trunk, at the very height which they had when they originated.

While upon this subject I wish to put upon record two measurements which relate to the rate of increase in diameter of old trunks of our American elm. Two large elms stand in one of the streets which bound the Botanic Garden at Cambridge. I carefully took their girth at six feet from the ground thirty years ago. One was then thirteen feet in girth; it is now fourteen feet five inches. The other was fourteen feet three inches; it is now fifteen feet five inches.

ON THE SUMMER DORMANCY OF THE LARVA OF PHYCIODES NYCTEIS (DOUBLEDAY), WITH REMARKS ON THE NATURAL HISTORY OF THE SPECIES. By C. V. RILEY, of St. Louis, Mo.

MR. S. H. SCUDDER, in the "American Naturalist" for September, 1873, published an interesting account of the curious history of a butterfly. The normal history of our butterflies is that one generation succeeds another either once, twice, or more often, annually; the species passing the Winter either in the egg, larva, chrysalis, or imago state.

The phenomena of dimorphism and polymorphism — the facts that a species may have two or more distinct forms of either sex, and that one generation may give birth to another which differs so much from the immediate parents as to have been looked upon as specifically distinct — have all combined to add unusual interest to the biological study of the butterflies. But Mr. Scudder, in the paper alluded to, has shown us something more remarkable than anything before known of these gaudy favorites among the insects. It appears that in the genus Brenthis there are, to use his language, "two sets of individuals, each following its own cycle of changes,

apparently with as little to do with the other set as if it were a different species: each set has its own distinct seasons, and thus gives rise to the apparition of two or three successive 'broods' in the course of the year." In one set the larvæ, which he terms the æstival series, hatch late in the Fall, devour their egg-shells, and hibernate; they produce butterflies the following July, and these lay eggs in September, which hatch and hibernate as their progenitors had done, and complete the normal annual cycle. The second set, which he calls the vernal series, hibernates as half-grown larvæ. These by the middle of May produce butterflies, and lay their eggs during the latter part of July. But the caterpillars from these eggs do not all develop alike; some go through all their transformations and produce butterflies which are killed by the autumnal blasts, and die without issue. It is a vain attempt toward the production of a second brood. The rest fall into a state of lethargy or premature hibernation, when half grown, and do not awaken till the following Spring, when they resume the changes peculiar to the vernal series.

After giving this account, Mr. Scudder refers to the case of a European species of the genus (*B. Euphrosyne*) which was observed by Vandouer to exhibit also a Summer larval dormancy. From eggs laid in May the larvæ hatched and fed for awhile. In June, however, they all fell into a state of torpor, from which a portion of them awoke in August and went through their transformations, while the rest did not awaken till the following Spring, when they produced butterflies at the same season of the year that their immediate parents appeared. We are not informed — and I presume it is not known — whether the butterflies which appeared in the Fall laid eggs or died without progeny, as in the case of the American congeners of the vernal series; but from the earliness of the season (August) when they attained the perfect state we may infer that they begat progeny.

The genus Phyciodes is closely allied to Brenthis, and a case came this year to my notice of Summer larval dormancy in *P. Nycteis*, which rather confirms the accuracy of Vandouer's observation, and renders the biological peculiarities of these small butterflies all the more complicated and their meaning all the more difficult to understand.

About the middle of June I found a brood of the young larvæ of this species on a leaf of *Rudbeckia laciniata*. These could

only have been hatched a few days, were gregarious, and strongly recalled, in general appearance, the young of the Heterocerous *Hemileuca maia* (Drury). They feed at this time only on the parenchyma of the leaf, leaving the transparent membrane; and they jump off with a sudden jerk when disturbed and fall to the ground in a curled position. Placing these in the vivarium, they all became lethargic after what was evidently the second molt, or about the first of July. A week afterward, however, two-thirds of them became active and voracious, and went through all their transformations, the first imago issuing July 20th, the last August 9th, the chrysalis state lasting from nine to fifteen days. Up to the time of my leaving home the rest remained huddled together in a dormant state, and will thus, in all probability, hibernate.[1]

Now the Summer dormancy of part of these larvæ corresponds to the facts recorded by Mr. Scudder as occurring in the vernal series of Brenthis; but it seems highly improbable that the imagines issuing as early as the 20th of July should die without issue. It is far more likely that they will lay eggs and that their larvæ, hibernating, will produce the Spring or early Summer brood of butterflies: and the only legitimate conclusion that I can draw from the facts so far observed is that the species in the latitude of St. Louis is normally double-brooded; but that retarded development occasionally occurs, allowing only one annual brood. What influences produce this check it is difficult to understand.

But such cases of retarded development are not uncommon in Lepidoptera. According to European observers, Empetheciæ normally remain two years in the pupa state, while among Bombycidæ, species which normally pass but one winter as pupæ (*e. g. Pœcilocampa populi* and *Eriogaster lanestris*) are recorded as occasionally passing two winters in the same condition. I have recorded the fact[2] that the Bombycid *Hemileuca maia*, which usually issues as a moth the fall after hatching, not only passes one winter in the pupa state quite frequently, but not rarely two winters in this dormant state. This occurs among the individuals of the same brood, treated alike, and under similar conditions in every respect; and I have shown how this irregularity in development may, and probably does, play an important part in perpetuating

[1] Since this was written they have remained dormant, and are still in that condition at the present time—Oct. 29th—C. V. R.

[2] 5th Mo. Ent. Rep., p. 132.

the species under conditions adverse and fatal to it in its more normal habit. Many other cases of protracted development in pupæ could be given, and there is certainly a parallel between abnormal summer dormancy of pupæ and the same manifestation in larvæ.

It would seem, therefore, that on the one hand we have in more southern latitudes a species normally double-brooded continually exhibiting a tendency to single-broodedness, and in more northern latitudes (for Mr. Scudder's observations were doubtless made in New England) a species normally single-brooded continually making a futile attempt toward double-broodedness. In other words, the exception in the south becomes the rule in the North, and *vice versa*, and the tendency to the exceptional extremes in either case results perhaps from the gradual intermigration of southern forms northward and northern forms southward.

There is a wide field here for future observation of parallel facts in allied species. There is also a large field for speculation for those of an evolutionary turn of mind. Before entering upon the latter it will be best to wait till the former has been more fully explored.

I append descriptions of the adolescent stages to complement those already given by Mr. J. A. Lintner.[3]

Phyciodes Nycteis (Doubl.). *Larva.* Average length, when full grown, 1·10 inches. Color velvety-brown, sparsely speckled with minute pale yellow papillæ, and with a broad lateral stripe varying from sulphur-yellow to orange and mottled with brown near each border; this stripe sometimes indicated only by three narrow lines, the middle one widest. Armed with black tubercles, from one-third to one-half as long as the joint from which they spring and furnished with verticillate, spinous hairs. These tubercles arranged as follows: on each of joints, 4–10 inclusive, seven of equal size in an almost direct transverse row, one medio-dorsal, two each side supra-stigmatal, and one sub-stigmatal—the spines on this last finer and pale; these joints also bear each side, two similar, but smaller, sub-ventral tubercles, longitudinally arranged on the proleg-bearing joints, transversely on the others. On the thoracic joints the medio-dorsal spines are obsolete (there being here a pale narrow line) the lower sub-ventral is sub-obsolete, the upper sub-ventral rather longer than on the others, and the sub-stigmatal obsolete on joint 3; while on joint 1 the cervical shield forms a small horny plate with two sets of three short, con-

[3] Ent. Contributions, I, p. 26, N. Y. St. Cab. Nat. Hist. 1869.

fluent tubercles, and two small isolated ones each side just above spiracle; joint 11 has an additional, medio-dorsal tubercle; joint 12 has but two, which are sub-dorsal, and the anal plate is also surmounted with two. Stigmata broadly-oval, and black. Venter pale with a few hairs. Legs black, with pale sutures. Prolegs with fuscous hooks and a fuscous spot at outer base. Head, small, polished black, strongly bilobed on top, sparsely furnished with soft hair, and with a few granular tubercles giving rise to stiffer hairs, most prominent on the top of each lobe.

Only three moults observed. In the first stage the color is brown and the appearance more hirsute, from the shorter, broader nature of the tubercles, and their more densely furnished ferruginous, spinous hairs. The deep brown is assumed in the second stage, the pale lateral stripe is more or less distinctly visible from the first.

Chrysalis. Average length 0·50 inch. Hanging from tip of body without other support. General form and structure of Argynnis. The ocellar tubercles not prominent, rounded, and but slightly ridged transversely. The shoulders transversely ridged. The cremaster forms, ventrally, on the last two joints, a slight, dark, oval ridge, almost on a plane with the axis of the body, enclosing a deep, oval, translucent cavity behind, and having two rather sharp tubercles in front, and three rounded, rugose ones each side behind; dorsally it is longitudinally depressed in the middle, with two small tubercles at tip; it is surmounted at extremity with a dense brush of brown hooklets. Color silvery-gray, more or less intensely shaded with brown mottlings. Dorsally with rounded, conical tubercles, black in front and white behind, as follows: two on middle of mesothorax; two, smaller, on metathorax, and on the abdominal joints replacing the spines of the larva, except that the dorsal ones on abdominal joints 1, 8 and 9 are obsolete, and indicated only by black spots, and that the lateral ones, except on joints 3 and 4 are similarly obsolete and similarly indicated. Other black spots and marks occur as follows: on the head two spots posteriorly and a broad mark between and upon the ocellar tubercles; four spots on prothorax, and two on anterior border of mesothorax; the wing cases are marked with conspicuous dashes on the shoulders, one on the disk, one toward apex and three near anal angle, and two series of smaller dots on the veins along posterior border; while the joints of legs and antennæ are also spotted.

Described from many specimens.

NOTES ON SARRACENIA VARIOLARIS. By J. H. MELLICHAMP, of Bluffton, South Carolina.

IN the early spring of this year Professor Gray having called the attention of observers to *S. variolaris*, I immediately commenced careful examinations of this plant, which have been continued up to the present time.

These examinations were made, not only for the purpose of verifying the observations of Dr. McBride of this state (made over a half century ago), as to a sugary exudation at the rim, but also to determine, if possible, whether this secretion possessed any intoxicating effect upon insects, thus assisting in their entrapment. The statements of Dr. McBride I was enabled to verify in every particular, while at the same time the continuation of the sweet exudation could be traced in another direction, as will be hereafter noticed.

Various experiments also have well satisfied me that the above secretion possesses no stupefying or intoxicating effect whatever, but that the watery fluid at the base of the leaf into which insects eventually tumble, *does* possess narcotic, or anæsthetic, or asphyxiating power, in a very remarkable degree!

Such notes as were made, illustrating the entomology as well as the botany of this very singular plant, have been from time to time communicated to Prof. Gray, at whose request I now venture to give them in full.

And in doing so I am not without the hope that other observers may be tempted to examine for themselves, not only this, but the various other species of *Sarracenia* found in our country; with the prospect of determining whether they are all honey-producing, and all secrete a watery fluid at the bottom of the tube; whether they all entrap insects, and to what extent; and whether they entrap and macerate such for their own sustenance alone, or for that of certain insects and their larvæ, or, to subserve both of these ends. In recording such observations as were made, I do so neither as botanist nor entomologist, but simply as an observer of Nature and her ways.

Sarracenia variolaris is so well known to botanists, that a description of the plant is unnecessary; it will suffice to state that this species is the only one growing in this locality, on the Carolina coast, and the only one that is yellow-flowered, save *S. flava*.

It is found mostly in our damp pine-lands, occasionally in wet bogs, but it flourishes best on slight elevations on the edges of "pine-barren ponds."

In such situations, with an open grassy pine-land surrounding them, with an abundance of sunlight and a sufficiency of moisture, these plants are found in large clusters or patches, their yellow flowers and curiously spotted leaves of green or purple, presenting an appearance as singular as beautiful. The leaves, which vary in length from one foot to eighteen inches, have certain peculiarities of conformation, the throat being covered by an "arched or vaulted appendix," a sort of upper lid, or hood, which extends forward and downward, overhanging the orifice and thus preventing the admission of rain water.

If the *inner portion* of the leaf be opened for inspection by slitting down the whole posterior border, three distinct divisions, or belts, may be observed. First, *the inner surface* of the hood or upper lid, marked on its posterior portion by white translucent spots and purple reticulations, which last extend forward and upward, and again downward on each side of the rim, for a half an inch, or sometimes an inch. This may be termed the *first belt*, and embraces the internal honey-bearing portion. Immediately below this, and extending for the space of three inches, there is an exquisitely soft and velvety pubescence, which under the glass is seen to be composed of very fine and thickly disposed retrorse hairs. This may be termed the *second belt*, and is so smooth as to afford no foothold for most insects. Below this again, the eye may detect a deeper colored pubescence, of a pale yellow or straw color, still smooth, but composed of coarser hairs, which become longer and more bristly as the tube narrows. *At the base of this tube* a watery fluid is secreted, into which insects are precipitated. This is the *third belt*, and is about five inches in length.[1]

On the 22d of April my first examinations of these plants were made, which at that time were blooming freely.

Most of the leaves were tender and immature, and I failed to detect the slightest indication of any sugary secretion about any portion of the leaf. I directed my attention therefore to the liquid at the bottom of the leaf, for the purpose of ascertaining its extent, and whether it was a true secretion of the plant, or only

[1] These divisions in inches are made upon the supposition that the whole leaf is about one foot in height. If the leaf be longer add more.

rain water, or a mixture of both. For this purpose I carefully examined many *young* leaves, with the throat still closed and impervious to water, and inflated as they usually are. Upon slight pressure the air would escape, thus opening the throat and a portion of the tube for inspection. The leaf being lifted, there was almost invariably an escape of "water," from three to five drops generally, occasionally as many as ten drops, and rarely fifteen drops. It is therefore a true secretion, as no rain could possibly have been admitted to the completely closed and sealed leaf.

The taste of this secretion was bland and somewhat mucilaginous, yet eventually leaving in the mouth a peculiar astringency, recalling very accurately the taste of the root, with which I am familiar. In these unopened and not yet matured leaves no trace of insects was to be found, either by puncture, or eggs, or larvæ, nor indeed any *débris* of any kind.

I next examined a great many perfect leaves with the throat open. In almost every leaf the secretion was to be found, containing generally from ten to fifteen drops, very rarely a half drachm. *I have, however, since found the fluid much increased in quantity*, very frequently a drachm, and *sometimes as much as two drachms*. Occasionally too I have seen the tubes half filled, and even filled to the brim, but evidently this was only rain water, and *not* the true secretion; for in these cases either the hood or upper lid had been torn aside, or mutilated in some way, or some portion of the leaf tube had been opened, by which rain could be admitted. Indeed, in a perfectly formed and mature leaf, the admission of rain water is next to impossible, so completely does the upper lid overhang and project beyond the mouth or throat, both in front and at the sides.

With very rare exceptions, dead and decaying or more properly macerated insects were to be found packed at the base of the tube, most frequently *ants*, both large and small; also beetles, flies, bugs, etc., and invariably within this decaying mass, *one or more small legless larvæ, which were very active, and seemed to exist upon the decomposing insects.* At this early period, most leaves contained only a small wad of macerated insects, chiefly ants, within which, the above mentioned minute larvæ were always to be found, and *over which, floated the still clear and almost tasteless liquor.* Some leaf tubes, however, were packed for the distance of an inch with various insects, in different stages of decomposition.

It may be as well here to mention, that on several occasions during my first visits to these plants, I witnessed the struggles of captured insects to escape, chiefly small beetles; but their efforts were always vain, as it seemed impossible for them to climb the smooth pubescence, and they invariably slipped downward, and eventually were precipitated into the pool below.

Not so, however, with a small moth, a pair of which were usually to be found in most well grown leaves, their heads always directed upward, and generally from two to four inches from the outlet. This moth is almost one half of an inch in length, and of a yellow or saffron color; the lower half of the wing being black, or at least of a darker color than other portions, and covered with thick feathers or scales. So frequently did I meet with these little insects during repeated examinations, that I at last looked upon them as denizens of the place; but at that early time it was impossible to conjecture their errand, or what was their quest; whether seeking protection from the heat in these cool chambers, or obtaining food, or depositing their eggs, it was impossible to say. One thing however seemed certain, that without let or hindrance they could move either up or down the treacherous walls of the prison house, and at their ease could enjoy the torments of their less fortunate brethren below.

There was one other insect frequently found within the leaf tube and occasionally associated with the moth, and this was a large bristle-covered fly. Unlike the little moth, upon being disturbed, he always attempted escape, which he effected apparently by his great strength. The strong bristles of his legs and body seem to support him against the velvety pubescence, and while using his wings incessantly, he would soon effect his escape. Once only did I find one of these flies entrapped in the lower tube, where he may have ventured unwarily. As with the moth, it was equally impossible at that time to conjecture the errand of the fly, and I could but watch and wait for what the summer months might disclose.

I afterwards sent both of these insects to that accurate entomologist and most courteous of correspondents, Prof. C. V. Riley of St. Louis, with notes, who informed me that the moth was most likely depositing her eggs, and that the larvæ would probably be found feeding upon the leaf of the plant. The name of this moth, as he informs me, is *Xanthoptera semicrocea* Guen.,

and the fly he will describe as *Sarcophaga Sarraceniæ* which, being viviparous, doubtless deposits its little maggots within the pitcher, which make their way to the decomposing mass below. Thus much was learned from first examinations, and thus much it seemed necessary to say by way of anticipation. I return now to the *watery liquid found at the bottom of the tube*, and to certain careful experiments with the same, *which have been frequently repeated up to the present time*. As I have *nothing further to add more conclusive* than the above experiments, as detailed at the time to Mr. Canby of Delaware, I quote from my letter to him in the following words: "By draining every leaf plucked of its few drops of juice, I collected about a half ounce of the secretion in a phial, with which I made careful experiments in testing its effect upon insects. My 'subjects' were chiefly house-flies. About a half drachm to a drachm of the liquid was placed in a small receptacle, and the flies thrown in from time to time, the liquor not being deep enough to immerse them completely, but enabling them to walk about in it without the risk of being drowned. Perhaps twenty flies were experimented with. At first the fly makes an effort to escape, though apparently he never uses his wings in doing so; the fluid, though not very tenacious, seems quickly to saturate them and so clings to them and clogs them as to render flight impossible. A fly when thrown in *pure water* is very apt to escape, as the fluid will 'run' from its wings, but none of these escape from the bath of the Sarracenia secretions. In their efforts to escape, they soon get unsteady in their movements, and tumble sometimes on their backs; recovering, they make more active and frantic efforts, but very quickly stupor seems to overtake them, and they then turn on their sides, either dead (as I at first supposed) or in profound anæsthesia.

I had no doubt from the complete cessation of motion, and from their soaked and saturated condition, that they were dead, and like dead men they were 'laid out,' from time to time, as they succumbed to the powerful liquor; but to my great surprise, after a longer or shorter interval, from a half hour to an hour or more, they indicated signs of returning life, by slight motions of the legs and body. Their recovery was very gradual, and eventually, when they crawled away, they seemed badly crippled and worsted by their Circean bath. After contact with the liquid, the flies *first* thrown in became still, seemingly dead, in about a half minute;

but whether from exposure to the air, or exhausted by action on these insects, the liquor did not seem to be so intoxicating with those *last* exposed to its influence. Anæsthesia or intoxication certainly did not occur so quickly; it took from three to five minutes generally, and in one rebellious 'subject,' it was at least ten minutes before he received his *coup de grâce*. A cockroach thrown in succumbed almost immediately, as did also a small moth, and much more slowly a common house spider. On the recovery of the latter, it was almost painful to witness his unsteady motions. Without doubt therefore, the secretion found in the tubes of *Sarracenia variolaris* is intoxicating, or narcotic, or anæsthetic, or by whatever word we may prefer to indicate that condition to which these small insects succumb."

As before stated, the above experiments have *been so frequently repeated up to the present, that I can have no doubt as to the correctness* of my conclusions. I regret that it has not been in my power to have this fluid carefully analyzed by an expert chemist; but I am unable to state its composition. It occurred to me, however, to test its digestive power, as it seemed not unlikely that the macerated and decomposed insects assisted in nourishing and sustaining the plant. I therefore immersed bits of fresh venison in this liquid, and also in a corresponding amount of pure water. After the lapse of fifteen hours, I found the venison more changed, softened and broken up in the former than the latter and also far more offensive to the nostrils, disgustingly so indeed;—as is the case still later with the leaf tubes, when they become packed with the accumulating mass of macerated insects for the space of two, three, and five inches, in the larger and more vigorous plants which have been most successful as "Fly Traps," as they are called here.[2] The experiment therefore proved, perhaps, only that it hastened the decomposition of insects, and the sooner converted them into "liquid manure."

Still later, during the first week of May, other examinations were made, and the existence of the sugary exudation noticed by Dr. McBride verified. I prefer to use the language of my letters to Prof. Gray, as follows: as I have but little to add to what was there stated. "Having discovered, therefore, in an unburned pine-land, many plants of *Sarracenia variolaris* which

[2] This is not always the case. I have lately examined some tubes whose contents were semi-liquid and having very little unpleasant odor. J. H. M., July 20.

were far more advanced than the more tender and succulent ones first examined, the leaves being more rigid, and harsher to the touch, and more deeply colored, I had no difficulty in detecting in almost every leaf the *sugary secretion or honey*-like exudation noticed by Dr. McBride as being found at *the mouth of the tube.* I find it precisely in the place described by him, save that it extended downward more than 'one quarter of an inch,' generally one-half an inch, or even three-quarters of an inch.[3] I also found it, but far more sparingly under the arched lid or upper lip of the leaf, in and among the thick and coarse hairs found there. Dr. McBride however *failed to trace the continuance of the sugary exudation*, which I frequently found, glistening and somewhat viscid, along the whole border or edging of the 'wing,'—extending from the cleft in the lower lip even to the ground. There is therefore a honey-baited pathway, leading directly from the ground itself up to the mouth, where it extends on each side as far as the 'commissures' of the lips, from which it runs within and downward, as before stated, for at least one-half an inch.

One now can readily understand why ants should so frequently be found among the earliest macerated insects at the base of the tube. Their fondness for saccharine juices is well known, and while reconnoitring at the base of the leaf, and bent on plunder, they are doubtless soon attracted by the sweets of this honeyed path lying right before them, along which they may eat as they march, until the mouth is reached, where, should they enter, certain destruction awaits them."

The sugary exudation noticed above can be seen in all leaves sufficiently mature, and favorably placed both as to sunlight and moisture; but I doubt not it might easily be overlooked by a careless observer: indeed some inferior plants fail to secrete it, or secrete it very sparingly. By holding up the leaf to the sun, the minute, glistening, and somewhat viscid drops can be distinctly seen, extending from the rim along the whole border of the wing to its lower extremity, and its sweetness and viscidity will be discovered both by the *taste* and the *touch.* I believe that it can be observed in most perfection in the morning from ten to eleven o'clock; for the sun evidently exerts its influence in hastening the secretion. About the middle of June I observed a clus-

[3] Even one inch in large and vigorous leaves. J. H. M., July 20.

ter of these plants growing in a still moist but fast drying pine-barren pond, which were exposed to the full blaze of the sun, and upon examination of the leaves, I found that the border of the wing, the honey trail, even *streamed over with the exudation!* This condition, however, I have not observed since, and it is doubtless unusual. I may remark that those leaves which show a deep red or purple colored border of the wing (as was the case above) secrete the honey most abundantly, and that those plants too much shaded by others, or dwarfed in any way from too dry or sandy a soil show it but slightly or not at all. As regards the duration of the exudation, although the border may retain its sweetness for some time, yet, to the eye at least, it is apparently rather evanescent,[4] perhaps not remaining over a day or two, certainly not over three or four days. It seems quickly to dry and harden (as does the honey within the leaf), so that unless the plants are examined during the period of active secretion, one would fail in detecting it, at least with the eye, but to the taste it would still be sweet for some time. Doubtless, insects, and especially ants, may detect the saccharine element long before it is visible to us, and may still be attracted by it as food, even after it has hardened and dried.

For the purpose of satisfying myself as to the stupefying or inebriating effect of this sugary exudation, so attractive to insects, I collected a large number of the most sugared and mature leaves and carried them home with me. These "I placed in two goblets of fresh water, and that morning fortunately having perfect leisure, I sat down before them, for the purpose of carefully watching the entrapment, and determining if possible, whether flies were intoxicated after sipping the honey. I remained in my position for at least two hours, and in other ways continued my observations more or less the greater part of the day. Flies were soon attracted to the leaves (but not so greedily as stated by Dr. McBride), and many were entrapped, the buzzing of the unfortunate prisoners being incessant. I soon found, however, that this was a poor way to test the matter, for although the flies would disappear, none escaping, it was no easy matter to see the process. I therefore turned backward the greater part of the overhanging lid or hood, and thus flooded the prisons with day-

[4] That is, in its viscid, glistening state, for as will be seen further on a *white powdery residue* is left over the pathway without and within in *vigorous* honey secreting leaves. J. H. M. July 20.

light so that the whole region of the 'sugar countries' could be seen and examined, while the flies were busy at their food.

Evidently Dr. McBride did not suspect any inebriating quality in the exudation, and from what I saw, I cannot agree with those observers who have attributed the capture and incarceration of the flies to stupor and intoxication! Very far from it: it seems to me that the nectar found at the orifice, and along the border of the wing, is simply *a lure*, and possesses no narcotizing effect whatever.

But what did I witness? After turning back the lids of most of the leaves, the flies would enter as before: a few alighting on the honeyed border of the wing, and walking upward, sipping as they went, would enter the mouth at the cleft of the lower lip. Others would light on the top of the lid, and then walk under the roof, feeding there; but most it seemed to me preferred to alight just at the 'commissures' of the lips, and either enter the tube immediately there, feeding downward upon the honey pastures, or would linger, sipping along the whole edge of the lower lip, and eventually enter near the cleft. After entering (which they usually do with great caution and circumspection apparently,) they begin again to feed, *but their foothold for some reason or other seems insecure*, and *they occasionally slip*, as it appears to me, upon this exquisitely soft and velvety 'declining pubescence.' (The nectar is not smeared over the whole of this surface, but as before remarked, seems disposed in separate little drops.) I have seen them regain their foothold after sipping, and continue to sip, but always moving slowly, and with apparent caution, as if aware that they were treading on dangerous ground. After sipping their fill, they frequently remain motionless, as if satiated with delight, and, in the usual self-congratulatory manner of flies, proceed to rub their legs (or hands) together, but in reality, as I supposed, to cleanse them.

It is then they betake themselves to flight, striking themselves against the opposite side of the prison-house, either upward or downward, generally the former. Obtaining no perch or foothold, they rebound from this velvety, 'microscopic *chevaux de frise*,' which lines the inner surface still lower, until, by a series of zigzag but generally downward falling flights, they finally reach the coarser and more bristly pubescence of the lower chamber, where, entangled somewhat, they struggle frantically (but by

no means drunk or stupefied), and eventually slide into the pool below, where, once becoming slimed and saturated with these Lethean waters, they cease from their labors. Even here, although they may cease to struggle and seem dead, like 'drowned flies,' yet are they only asphyxiated, not by the nectar at the rim, but by this 'cool and animating fluid, limpid as the morning dew,' on which Bartram descants. After continued asphyxia, they die, and after maceration they doubtless add to the vigor and sustenance of the plant. And this would seem to be the true use of this 'limpid fluid,'—not to kill them (although it does possess that power), for *the conformation of the Fly Trap is quite sufficient for that purpose.* By being plunged in this liquor *they die the sooner*, and the sooner become 'liquid manure,' through its solvent and macerating qualities.

From this digression I return to the entrapment of the flies. As before stated I could never see any indication of tottering or unsteadiness in the sipping flies, nothing save an occasional slip from the uncertain hold which the peculiar pubescence would give, save once or twice while watching intently I saw a fly disappear so quickly downward, that I could not with certainty say whether it was flight, or a tumble from stupor or insensibility. But on so many other occasions have I satisfied myself to the contrary, by seeing them fly upward as well as downward, with the full vigor of an unhurt unintoxicated insect, that I altogether reject the idea of stupor.

I may state also that while watching, I observed not a single escape when the lid was down, but that after I had turned these back on most of the leaves under examination, a few, but only a few, escaped. And those which escaped after sipping to repletion seemed in nowise inebriated.

Indeed, I have still better proof as to the innocuousness of the nectar. About sunset I brought home perhaps a hundred leaves, all maturing and sweet with the exudation. The honey-bearing portions were torn off, and being placed on a table after candle light they soon attracted a few hungry flies, which seemed to enjoy the evening meal thus afforded them. Of course there could be no entrapment here, but if the nectar were intoxicating (as was said to be the case) the flies would soon show it either by unsteadiness or tumbling, as well without, as within, the tube. I can only say that they continued to eat, but no tottering or falling

was in a single instance to be seen. The same experiment was tried the next day, and on several occasions afterwards, so that I deemed it unnecessary to test the matter any further, being quite satisfied of the innocuousness of the sugary secretion.

To meet the suggestion that had been made to me, that although insects entering the leaves might not be stupefied by the honey which they eat, yet, that it was possible they might succumb to a kind of poisonous exhalation from the liquid in the pool below (which had been proven to be anæsthetic), the following experiment was made. Two house-flies enclosed in thin gauze were suspended *within* a large open-mouthed phial, at the bottom of which was about a half ounce of the pure secretion gathered from the plants. They were placed within the phial at two o'clock, P. M., and when examined that night, about nine or ten o'clock, they were still frantically active in their efforts to escape. The next morning I found them dead, evidently from confinement and exhaustion. Two flies were next placed in the same gauze, and hung up in the house in a cool place, and these also in about the same time shared the fate of the former. I infer, therefore, that although the fluid is narcotic to flies or insects, when immersed in it, yet that there is no stupefying exhalation arising from it whatever.

Having satisfied myself that the sugary exudation had no inebriating effect upon flies, and that they failed to escape, simply because they could not climb out of their prison, I commenced similar experiments with ants. I first tore off the sugary portions of many leaves, strewing them upon the outer rim of a nest of large red ants, so common in our sandy pine-lands. The foragers and outrunners soon detected the intrusive morsels, and communications being rapidly established with head-quarters, the nest was soon in a ferment, and in a moment, parties were either tugging the leaves to and fro, or more wisely engaged in eating the sugar. The motions of these insects I watched for a long time, but as in the case of the flies I could see no indications of stupor or intoxication.

I did however observe that two of them became unsteady, and at last even turned on their sides, and able to walk only with difficulty. I soon discovered however that their legs were clogged and sticky with the exudation, and that in this way their motions were impeded. But, when they had extricated and cleansed

them they immediately lost their unsteadiness, and although I afterwards carefully watched them, I saw no return to this condition. I next placed some leaves in an erect position, amid the black cinders of burned pine bark, with which they bulwark their home. A few of the more venturesome rapidly climbed to the rim, 'for to see,' in the usually frenzied and nervously restless style of such folk, and they were immediately precipitated into the pool below. Others more cautiously went in, remaining near the orifice, in and among the honey pastures, and came out again; but others still, venturing unwarily upon the smooth pubescence, soon shared the fate of the first. The last comers, however, gratified their curiosity by only 'peeping in,' and contented themselves with seeing, but not sharing the struggles of their comrades below. The same experiments were tried with colonies of the smaller ant, both red and black, and with the same results.

Doubtless, not all of the insects entrapped are attracted by the exudation, for many of these, as I am informed by Mr. Riley, do not care for sweets. What then do they seek? The night season seems equally propitious to the capture of some of them, as the day to others. Is it protection from other insects? or the decomposing mass at the bottom of the leaf? or is it the watery liquid? or is it curiosity? Let no one smile at the fancy, who remembers the ingenious devices of duck-shooters, or the hunters of our western prairies, in appealing to this well known 'trait' of birds and other animals. But this matter, the entomologists may decide for themselves.

Is it not almost a misnomer to call these plants 'Fly Catchers,' and 'Fly Traps', when, after all, they catch but few flies, that is if by that word we refer to the dipterous insects found in our homes, as well as those in the woods? True, if a bunch of *S. variolaris* be hung up in a house, numbers of flies may be entrapped, but in the woods, few or none of these would be caught, and not a great many of other varieties. If a tube of Sarracenia be opened one is almost certain to find, *first*, the limbs and macerated portions of several kinds of ants, both great and small; indeed, in some old and exhausted leaves, I have seen a dry powder composed almost entirely of the *débris* of such insects, and extending up the tube, for two or three inches. Beetles, usually of the smaller kinds, are common enough, sometimes crickets, occasionally grasshoppers, and on one occasion I found what I took to be the body and legs of a large katydid.

On another occasion I was no less surprised to find a leaf-tube blocked midway by a large snail, and once, just above the reeking mass, the gay wings of that 'dandy of the summer woods' the *Deiopea bella*.[5] Evidently then are some insects entrapped, which are not allured by the sweet exudation.

As regards the usual proportion of captured insects, the leaves appear to differ much, depending possibly on the size and vigor of the leaf and the amount of the sweet secretion. All leaves favorably placed would at least contain one inch of captured insects, usually I would say three inches, sometimes, or rather I should say frequently, four inches, and very rarely as much as six inches. And as for the watery liquid in which they are dissolved as before stated, I do not think that it often exceeds two drachms, while generally it is much less. The exact amount secreted it is of course difficult to arrive at accurately, for when a leaf has been successful in its captures, only a semi-liquid mass will be found at the base, which will not pour; for when tilted, but a few dark and turbid drops issue from the mouth. This secretion nevertheless appears to continue during the whole period of the entrapment, which I suppose does not last over two or three weeks at most.

During this active period, the 'water' is attracted to the top of the mass and permeates and percolates through every portion of it so that eventually all the soft parts of the insects are thoroughly dissolved. After these animal juices have been partially, or entirely absorbed by the plant, or by the larva of the Sarcophaga fly which continues to grow and fatten upon this rich diet, the remaining portion, as before alluded to, commences gradually to dry, until only the backs and legs and shells of the various insects remain.

To what then are we to attribute the capture of insects, if not in the first instance to the attracting lure, and the overhanging lid, and above all, to the slippery pubescence of the narrowing funnel, which only the little saffron colored moth and the bristly Sarracenia fly may surmount? As for the conformation of the hood or lid, it is by no means strange that this alone should often prove a bar to the escape of all winged insects, which may have been caught.

[5] Rather I should say what I take to be *Deiopea bella*; on the 14th of July I found the same beautiful insect in another tube in exactly the same position. This specimen as also that enclosing the snail I now have in my possession.—J. H. M.

When a fly, for instance, has been satiated with the sweets of the inner portion of the tube and he makes for flight, he must do so immediately upward for a very short distance, and then turn quickly at right angles, to get through the outlet; a somewhat difficult flight, of which perhaps of all insects a fly might be capable, but which even *he* probably is not. If he attempt flight, he is very apt to strike the arch overhead, and if he escape this, it is next to an impossibility for him to turn and enter the small space between the downward projecting lid and the lower lip. If his head should have been directed downward when making for flight, he is very apt to strike the opposite wall at a still lower angle, and then, from rebound to rebound, get lower and lower, until he touches the pool. In almost every instance therefore a fly once entering would be caught. Besides, is it too wild a fancy to suppose that these white diaphanous spots, which so beautifully mark the upper and posterior portion of the leaf, may play some part in misdirecting the flight of insects when attempting escape?

Doubtless this may be esteemed a 'vain imagination' yet, while watching the motions of flies engaged in my first experiments, I have seen them strike against these 'sky lights,' over and over.

I return to the peculiar pubescence of the tube, so fatal to the entrapped insects, and it may not be out of place to relate what seemed a very curious fact.

Upon opening the whole posterior portion of a leaf, and turning out the semi-fluid contents at the bottom of the tube, a fly was observed so saturated with the turbid liquid, as to be unable to use its wings. It soon, however, regained sufficient strength to use its legs with vigor, and with its head directed to the outlet, it was placed in the middle third of the tube, which happened to be slimed with the solution. The tube was then placed perfectly *flat* upon a table.

The fly immediately made an effort to advance, but to my great surprise its most vigorous and persistent efforts availed nothing, as it slowly, but steadily, retrograded to the lower extremity of the tube! The experiment was repeated frequently, but always with the same result. It was as if a boat with insufficient propulsive power were steadily drifted back by a strong tide, only in this instance the tide seemed to be the polished retrorse hairs, made still more slippery by the fluid, with which also the insect was covered. Are these wonderfully smooth hairs elastic, which might

thus assist still more in pushing backward and downward the insects attempting to surmount them?

Within another tube a beetle was found unable to fly, yet equal to considerable exertion with its feet. The tube was opened as the last, slimed, and placed flat. After great difficulty he succeeded in reaching the top, but upon the slightest elevation of the leaf he invariably slipped downwards, so that the labors of Sisyphus are repeated, again and again, in these treacherous and deceptive leaf tubes.

It has always been a matter of some surprise to me, that although these plants are at times so freely bedewed with the honeyed secretion, and on that account so attractive to many insects, I should have looked almost in vain for the presence of the true honey gatherer, "the little busy bee," either upon the flowers in early spring, or still later upon the sugared leaves. Only on one occasion did I find the bulky body of the large humble bee blocking up a leaf tube, and once only I shook out from a bunch of the leaves a half drowsed honey bee, which seemed to have been immersed in the fluid. The absence of bees seemed the more remarkable, as an odor of honey sometimes issues from these plants when growing in large clusters, and in favorable situations. This had not been noticed in my previous examinations here, but on the 24th of June, having been called to another locality, I observed on the edge of an open pine barren pond a large patch of these plants, which were unsurpassed for size and beauty. A few of the leaves were twenty-one inches in length, and were most of them matured, the sweet exudation in its viscid state having long since disappeared from most of them; yet so great evidently had been the amount of the secretion that its fragrance still clung and was unmistakable, reminding me of the strong honey-like odor of *Andromeda nitida*. But this shrub had bloomed long ago in March and April, and the only flowers near were those of *Rhexia glabella* and *Kalmia hirsuta*, neither of which is fragrant. As for odorous forest trees there were none save those "dreamy Titans"[6] of our pine lands, the long-leaved pine and Elliott's pine, whose health-bearing exhalations could hardly be mistaken for the fragrance of honey.

It was here that the only honey bee was found, and here for the first time I observed that in all of these plants the whole border

[6] Paul H. Hayne, "Forest Pictures."

of the wing from the ground up to the tip, and within wherever the honey had been secreted, was covered with a white or ashen colored residue, which I at first mistook for the thin filmy web or carpeting of either the purple banded moth larva, or some other more minute insect. Mr. Riley, however, to whom I sent specimens of the above, informs me that under the microscope he could detect no threads, and therefore supposes it to be the "residue of the honey path," which indeed I doubt not it is, especially as it is still a little sweetish to the taste.

I stated that during my first examinations of the plant about the first of April or the first week of May, my attention had been especially attracted to the little yellow moth (*Xanthoptera semicrocea*) from the fact of its being so frequently seen within the leaves, and I watched it with the more care as it seemed not unlikely that the fertilization of the flowers was effected by its agency.

In a letter received about this time from that excellent botanist, Mr. Canby of Delaware, he refers to the peculiar structure of the flower, especially to the fact that when the pollen falls it must necessarily do so within the inverted 5-angled parasol, and that it would seem impossible for it to reach the stigmatic surface (which is on the outside of the little turned up tips of the same) unless by insect agency. He alludes to the fact also that the lobes of this umbrella being filled by the petals on the outside, must necessarily prevent the ingress of an insect there, but permitting it in a little opening just above the stigma. A bee or larger fly entering just over the stigma would necessarily get dusted with pollen, but comes out without dusting that stigma which bends with its weight, and is not placed so as to catch it. But in entering the next flower at the opening he would probably rub his ventral surface thus covered with pollen directly against the stigma, and thus cross fertilization be effected. And this does not seem unlikely or at all improbable, remembering as we do the discoveries and investigations of Mr. Darwin, or the wonderful agency of the little moth Pronuba in fertilizing the flowers of Yucca for the sake of sustaining its larvæ upon the capsular seeds. The only insect thus far discovered within the flowers of *S. variolaris* is a small beetle, *Euryomia melancholica*, which on several occasions I found completely hidden and buried in and among the stamens, and he doubtless is the effective agent. As for our little yellow

moth, his downy scales are so easily brushed away that he would seem too frail for so rude a work, and indeed I have never yet seen him venture without the tube, unless the leaf be plucked and very roughly handled. Certainly, I have never seen him within a flower, and if he ever leaves the leaf-tube it must be either at twilight, or during the night, when it is not easy for me to continue my examinations. As for food, I think they have been already furnished with that, in the just forming sweet exudation, which on one occasion I saw them eating. The first "crop" of these moths is, as before stated, large, but they are not seen over two weeks, the lineal heir succeeding, taking its place in the shape of a very beautiful white and purple banded larva, which commences to be seen about the last week of May or the first week in June, at which time they begin to feed on the leaves. This larva is about one inch in length, has six thoracic legs and four abdominal, and I believe two anal prolegs. In walking it makes the usual arch of such larvæ, with a peculiar wavering and trembling motion of the head, which is marked with white and black spots. Having placed a cluster of these plants in a box on my piazza I had an opportunity of watching the larvæ. When small they begin to nip a little, generally about the honey pastures, or a little lower, but after gaining a little in size and strength, they spin a very fine gossamer-like web, just at the mouth of the tube, thus connecting the hood and lower lip, and by which they effectually bar the outlet against all intruders from the outer world. They then go to work in earnest, eating both the anterior and posterior portions of the upper third of the leaf through the parenchyma, and leaving only the thin epidermis. Perhaps the closest cropping in nature. The smaller leaves will sometimes be almost entirely devoured, but in the larger, only the upper third, and in some of these, only small patches, chiefly the sugary portions. A cluster of these plants at this season, formerly so erect and fresh, and green, now looks burnt and blasted; the upper portions of leaves frequently collapsing and falling down, while all that seems to support them is the continuous packing of the tube lower down with the excrementitious deposit of the larvæ. This mass, therefore, is often superimposed upon that of the decomposing insects still lower down, but frequently there is a vacant space of a few inches between them. I have not been able to satisfy myself very accurately as to the period of the larval life, but having furnished

Mr. Riley with specimens both of the larvæ and pupæ in various stages, it is not unlikely that he has determined this and other points with more accuracy than was possible to me. I will only state that about the middle of June, earlier or later, the larvæ assume the chrysalid state, hanging by their webs either above the sawdust-looking deposit, or just within it. About the third week in June, a little earlier or later, the *second* crop of moths appears, but they are far less abundant than when first observed. I conclude that they continue during the summer as long as there are leaves to be eaten, or (as is the case sometimes with our cotton caterpillar) until caught by the first frosts of the winter.

Having called the attention of Mr. Riley to the fact that while other insects were unable to climb the smooth inner surface of the tubes, this little moth moved with facility either up or down, he states that "there is nothing peculiar in the structure of the moth's legs, and that there is no reason why other moths could not with impunity crawl over the inner Sarracenia surface," from the fact that "most moths have the shanks and thighs armed with spurs or spines, or with both, of varying length, by which, compared to other insects not possessed of them, they are able to move with freedom and rest tranquilly on such a surface as Sarracenia presents. In *Xanthoptera* not only are the hind tarsi armed with sharp thorns or spines, but the three pair of spurs (one pair on each middle and two pair on each hind tibia) are very long, and shod at tip with a sharp point, which acts like the spike on an Alpine traveller's staff." As for the larva of the above, he says that it "may easily defy the *chevaux de frise*, by bridging it over with a carpet of silk."

Of the fly, noticed before as being sometimes associated with the little moth, and which Mr. Riley has named *Sarcophaga Sarraceniæ*, I myself have but little to say. Upon inquiry of him, "why should the common house fly slip upon the inner surface of Sarracenia, and fall into the pool below, while this bristly covered one should be able to go in and out with impunity," I received from him the following notes, which will be read with interest on account of the ingenious explanation offered.

He first calls attention to the fact, which most persons have observed, that the common house fly frequently slips even when crawling up a window, and that for the same reason they may slip upon the smoother portions of Sarracenia just below the mouth.

"The last joint of the tarsus has two movable claws, very sharp pointed, and a pair of pads or 'pulvilli.' These pulvilli were formerly supposed to operate as suckers, but in reality they are thickly furnished with hairs, which terminate in a minute expansion, which is kept moist by an exuding fluid. It is by means of these innumerable hairs with disk-like extremities, and the two hooks which cling to the slightest inequality of surface, that the fly walks on the smoothest surface, and the pads or cushions perform (besides being a support to the hairs) the same function as those of a cat, in protecting the claws and preventing their abrasion. Now the retrorse pubescence, even where it is least perceptible, is sufficient to prevent all these fine disk-bearing hairs from touching ground; and the claws, from the direction of the pubescence, will only get good hold when the fly walks down the tube. I think this alone will account for the unsteady gait and downward tendency not only of the house fly but of the other insects, for I must doubt if they are influenced by the liquid until they touch it. Now, if you compare the tarsus of our common house fly (*Musca*) with that of *Sarcophaga*, you will notice that the latter is much more strongly armed with stout bristles, and that the claws, and consequently their pads, are much larger; they are, in short, long enough to touch ground, *i.e.*, to reach the cellular tissue of the leaf through the setous pubescence." * * *

As for the maggot, the legless larva of the *Sarcophaga Sarraceniæ*, which even in April I found, frequently three or four or more, in every leaf tube (although the little wad of macerated insects containing them was occasionally but little larger than a buck-shot), it continues to grow and fatten on its strong food, rioting upon the decomposing mass until, at the end of the season (June and July), but one remains, the sole survivor of the internecine conflict. It is the old story of the survival of the fittest, and "nature" is still "red in tooth and claw." Having thus derived its growth and strength from its weaker brethren, as well as from the semi-liquid mass of insects surrounding it, it becomes a large white maggot, when full-grown over one-half inch or about three-quarters of an inch in length. If the base of a leaf-tube be at this time examined, it will be observed to be turning black, or decaying, from the contents so long enclosed, and occasionally an orifice is seen just above this decay, through which it is not unlikely the larva emerges, immediately entering the earth there,

and eventually assuming (as Mr. Riley informed me it would) a coarctate form and of a brown color. Evidently, however, many of these larvæ fail to escape and to undergo their transformation, as at this period, when they are maturing, I have frequently seen numbers of leaf-tubes stripped to shreds at the base, as if by external violence, possibly by birds seeking for "worms;" and may not this be the work of the partridge (quail), which at that time I frequently saw in the neighborhood of these plants?

I had thus carefully watched the various stages of the brief life of these two insects, the little moth and the bristly fly, from early spring up to midsummer, with the hope that I might offer some explanation of their invariable attendance on these plants. But there is no proof, as before stated, that the fertilization of the flowers is due either to the one or the other. The bristly fly will buzz around the leaves from day to day, and delights especially to bask upon their summits; but I have never seen it enter the flowers; only the melancholy chafer, *Euryomia melancholia*, as determined by Mr. Riley, is found there.

What then? Does our Sarracenia catch flies for mere sport, or for its own sustenance alone, and what part does the large bristle-covered fly play in our desolate pine-lands, that so ingenious a contrivance should exist for the nourishment and growth of her young? Or has the little yellow moth no other task in early spring than depositing her eggs in a plant, that affords so safe a retreat for herself, and so nourishing a food for her larvæ? But let us content ourselves with *facts* only, so far as we know them.

It would seem that these insects are but parasites upon the plant, one of them being only a hunter of carrion, and if not feeding upon it, at least industriously seeking it for the sake of its young.

Such are the facts elicited by careful observation during the past few months, and if there be mystery, let others trace the meaning of it. These are among those things not yet dreamed of in our philosophy; but, if it be indeed true that "nothing walks with aimless feet," we may yet hope by patient watching of nature to master some of those wondrous secrets, which now she so jealously hides from us.

Note.—It may not be out of place to state that the root of this plant (as also that of *S. flava*) enjoys some reputation in this state, as well as in Georgia, in the treatment of dyspepsia, gas-

tralgia, pyrosis, etc. It has probably bitter-tonic and stomachic properties. See article on the medicinal and chemical properties of the plant by Prof. F. Peyre Porcher of Charleston, S. C., in the "Charleston Medical Journal," January No., 1849.

THE LOBSTER (*Homarus Americanus*) — EXTENT OF THE FISHERY —THE SPAWNING SEASON—FOOD OF THE LOBSTER—SHEDDING OF THE SHELL—LEGISLATION ON THE FISHERY. By WILLIAM W. WHEILDON, of Concord, Mass.

THE extent of the lobster fishery at the present time, and the fact that legislation has become necessary for its preservation, give the subject special interest. No less than eight thousand tons of lobsters, it was stated before a committee of the legislature of Maine, were used at the canning establishments in that State in 1873; and the expenditure for the year was at least one and a half million of dollars. It was testified by several witnesses that more lobsters were taken in 1873, than ever before in one year, though generally of small size.

THE SPAWNING SEASON. It is assumed that the lobster has a spawning season and this has heretofore been considered to be in the months of June, July and August, with more or less variation from north to south; but this is by no means certain, since, during the last year, it has been possible to obtain spawn in different stages of development in every month in the year,[1] while it may be somewhat more abundant in some months than in others. In February of the present year, we exhibited spawn in several stages of development from newly laid eggs to the swimming larvæ. It seems to be certain, we think, that there is no particular spawning season, nor any evidence to show that each lobster has an annual season, though there may be approximate periodicity. It is

[1] This we have done.

believed that some may even spawn more frequently, without regard to season. An English authority, many years ago, said "lobsters continue to spawn as long as coral can be found in their bodies," and also that "hen lobsters are found in berry at all times of year," but these statements were not generally received. We have found lobsters carrying spawn and at the same time containing what appeared to be a new formation of "coral."

Food of the Lobster. The statement also that the food of the lobster is "seaweed, filth and garbage" is untrue and militates with the statement that they live "upon rocky, gravelly and sandy bottoms." An experienced lobster man says that "though voracious, lobsters are epicures in the choice of food and are not easily enticed into the trap excepting with the freshest of bait." They are undoubtedly, to a certain extent, cannibalistic in their habits, and are continually preyed upon by various kinds of fish, large and small. Lobsters have been known to attain a very large size, up to forty-two pounds weight. Formerly the average was about four pounds; now it is hardly more than two pounds.

Shedding of the Shell. One of the most interesting peculiarities of the lobster is the casting of its shell at different periods of its life and growth. In the early stages of its existence, the young lobster is without a shell, and it is not precisely known at what age the first shell is formed or how frequently changed. The statement is made that the lobster changes its shell annually, and some of the lobster fishermen have expressed themselves as very confident on this point; others have gone so far as to determine the months of the year when this important change takes place. The statement in Ree's Cyclopædia is that "lobsters change their shells annually like other crustaceous animals;" and also that "the female lobster does not cast her shell the same year that she deposits her ova, or, in the common phrase, the berry." We have doubts as to the correctness of these statements: the first, so far as confined to any season, we consider wholly erroneous; and the second, whatever probability there may be about it in itself, we think is equally incorrect.[2]

[2] Mr. Johnson assures us that he has seen lobsters carrying ova a week or two only before they would shed their shells; and we have found coral in lobsters which had shed their shells within two weeks.

Lobsters which have just shed their shells, or are soon liable to cast them, are seldom brought to market, and this is undoubtedly the reason why more is not known in regard to the process and its peculiarities. When in this state they are popularly called "shedders," and, are supposed to be unwholesome or unpalatable as food; and whether so or not, they are never exposed for sale or eaten. They are thrown back into the water or used as bait by fishermen, and very often in lobster traps. Fishermen do not hesitate to say that a short time before they shed their shells, lobsters are in their best condition for the table. Soft-shelled lobsters may be found at all seasons of the year, and are hardly more abundant in the warm months than in some cold months; so that should it prove to be true that they shed their shells annually, there can be no common time or season for their doing so. In the soft state they are found in places of security, among the rocks, in comparatively shoal water; but are often taken in the traps, not only after they have cast the shell, but just before doing so, showing that they are still feeding, and not pining or shrinking away. A lobster fisherman at Portland, in February, 1874, stated that he had found no less than 400 soft-shelled lobsters in his last fare, a few days previously — a number much larger, we think, than is often taken at any one time in the summer months. An old lobster-man says that, on one occasion, while feeling around in a pool of water near the shore, his stick was seized by a male lobster which he found was engaged in guarding a female lobster while in the soft-shelled state. Soft-shelled lobsters are preyed upon by numerous kinds of fish, if not in fact, by other lobsters.

At the time of shedding the shell the lobster is popularly believed to be much reduced in flesh, which is said to be soft and watery; but this condition of things, if it exist at all, is much less than the public have been led to believe. We can only say that we have seen no evidence of it, but the contrary. Two male lobsters, in a condition to shed their shells in the course of two or three days, were boiled on the 3d day of August current, and they were not only full of flesh, but it was quite solid and in good condition. The claws especially were full-meated, and the only peculiarity noticed was that the flesh appeared to be somewhat dry and crumbly.

The new shell of the lobster, at first a mere skin of dark-green color, is formed underneath the shell, and becomes red when

boiled. It increases rapidly after the old shell is cast, and in a short time is fully established.[3] The statement that "lobsters have been known, when captured, to effect their escape by slipping out and leaving their shells in the hands of the astonished fisherman," is simply an exaggeration. Shedders are very apt to be quiet, almost dormant, and not at all capricious. Lobsters, as is generally believed, have the power of throwing off either of the large claws in case of necessity by reason of entanglement or otherwise, and it is said they sometimes do so in case of fright, or by reason of heavy thunder or the firing of cannon; but this is not done, as generally understood from the ordinary statement, by the dislocation of a joint; it is effected by an absolute break of the shell of the claw at a particular point, below the small joint near the body. The place is distinctly indicated in the formation of the shell at that part of the claw, by a slight ridge around it. When the shell is broken off at this point, a new claw is produced — though rarely as perfect as the original claw; but if dislocated at the joint, it is believed a new claw will not grow. It is very common to take lobsters having only one claw, and in many of the cases a small new claw may be seen protruding from the thin membrane which covers the break, sometimes not larger than a kernel of rice. A very interesting question here arises as to the growth of the new claw and the changes of its shell. For a time it is fleshy and grows without a shell, and thereafter, it would seem, must conform its growth and change of shell to those of the lobster.

Lobsters, it is known, drop off their claws in freezing weather; and fishermen say they never catch shedders having only one claw, and for this reason think they do not shed their shells although in good flesh. How then does the claw grow?

Since the enactment of a law in Massachusetts for the regulation of the lobster fishery, it has become necessary to measure by the eye or otherwise, the lobsters taken. Heretofore it has been the general custom to weigh them; and they have been sold either by weight or count. In July last, Mr. Geo. Wadsworth, a lobster fisherman of many years' experience, found in one of his traps a

[3] Since reading this paper we have had some evidence of the rapid thickening of the new shell. When taken the lobster was nearly ready to shed its shell. It was placed in a box in the river, and soon cast its shell. Two days afterwards the lobster was boiled; the shell had the natural appearance, but was thin, apparently about half the thickness of the ordinary shell, and crimped or wrinkled in drying.

lobster which had cast its shell in the trap. Upon measuring the shell in the manner provided by law, from the head to the end of the tail (exclusive of the claws and feelers), it was found to be exactly ten and a half inches in length, while the living lobster in his new envelope, measured in the same manner, was found to be twelve inches in length, and the measure around the body was found to be three-quarters of an inch greater than that around the shell; thus showing a growth which had made necessary the change of shell, and which added to the difficulty of his escape from the old one. In another case, similar to that just stated, the lobster was found to be one inch and three-quarters longer than the shell which it had just left. The flesh in these cases seemed to be quite full, though perhaps a little watery, and looked as if it might be easily elongated or stretched, and be thus made smaller in thickness.

The change of shell, as already intimated, is a necessity of growth and dependent upon it; and growth of course must in a measure be dependent upon the abundance of food and the activity of each lobster in obtaining it, which forbids the idea of an annual or periodic change as a rule. There is much diversity of character in the lobster as to vigor, strength, activity and growth; and those most vigorous and strong, it would seem, must change their shells more frequently and earlier in the season, than others less vigorous and less rapid growers. These would probably come under Darwin's classification of "natural selection." They are always the leaders of the column in any movement from place to place.

It is not improbable that, as an approximate rule, lobsters shed their shells once a year — it may be earlier or later — but not regularly, either as regards the species or individuals; and this begins at a certain age and may likewise end at a certain age, neither of which is known. There are months in the year when shedders are scarce, and other months when they are very plentiful, but this rule may not hold good for a series of years, even in the same months, especially as their breeding habits as well as other habits may possibly be disturbed by the present system of catching such a large proportion of the lobsters that are fit to be eaten as food each season. In the present year, as we have seen, in January and February, lobsters were plentiful, shedders were numerous in some places at least, and spawn was to be had in abun-

dance in different stages of development. In May and June, the fares of the fishermen were not as large as in July and August, for the reason that there were many shedders and recent shedders, too small to be brought to market; many of those which were thrown back in May and June were retaken in July and August, when they were large enough for market. Half the catch in July and August were recent shedders — in fact, nearly all the lobsters taken through the year are recent shedders; and this will probably be the case while the present system of fishing is continued, as every lobster fit for the market — as far as possible — is taken; and this again will probably continue to be the case whatever size may be prescribed by law. Hereafter, we conclude, large lobsters of several years' growth, are hardly to be looked for in the market.

The process of shedding the shell is known to fishermen and naturalists, excepting perhaps in one particular, which, so far as we know, has never been explained. The shell opens in a straight line in the length of the back of the lobster, while the tail, legs and claws are drawn out from the shell, leaving it entire: an "articulated skeleton," as it has been correctly called. "In casting their shells," says Ree's Cyclopædia, art. Cancer, "it is hard to conceive how the lobsters are able to draw the flesh of their large claws out, leaving the shell entire and attached to the body; in which state they are constantly found. The fishermen say" (continues this account) "that the lobsters pine before casting till the flesh of the claws is no thicker than the quill of a goose, which enables it to draw its parts through the joints and narrow passage near the trunk." This has always been a mystery.

Upon reading this statement, it will very likely occur to those familiar with the structure of the lobster that this difficulty is enhanced by the existence of a thin, bony or cartilaginous blade in each of the large claws by which the thumb is attached. This blade extends into the flesh of the claw flatwise, and the flesh of the thickest part adheres to it with much tenacity. It seemed to us impossible that this bone could shrink so as to be drawn out with the claw as described. This circumstance led to an examination of the claw and the process; and it was ascertained that this cartilage and others of less size are not drawn from the shell but from the flesh, and remain attached to the shell. [Since the reading of this paper this fact has been repeatedly con-

firmed, so that there can be no doubt about it: the investigation, however, led to the explanation which follows.]

The difficulty in withdrawing the claw, however, still remained; and it seemed impossible to deny the statement above quoted, incredible as it appeared after what we had learned of the condition of the lobster; but fortunately we are now able to show how by a simple and peculiar, if not *unique*, provision of nature, the operation is effected. If careful examination be made of that portion of the shell of the large claws nearest the body, which rests flatly upon the trunk, near to the thimble-like joint — which is by far the smallest part of the claw — it will be observed to have on the under side some peculiar markings in circular lines, within which the shell will be seen to be thin and possibly slightly discolored, especially if near the time of shedding. The portion of the shell indicated is smooth, comparatively thin, easily pressed in upon the flesh, and defined by the lines mentioned. It is nearly oval in form, and after the shedding can only be described as a hole in the shell, which greatly enlarges the passage-way for the claw. In lobsters of the average size of those now brought to market, this portion will be about covered by the thumb when pressed upon it. When the lobster is prepared to shed its shell, this portion becomes by absorption a thin, semi-transparent membrane which is easily broken, leaving the opening described and a fragile bony ring around the thimble-like joint which attaches the claw to the body; and through this ring (which is often broken) the flesh of the large claws must be drawn. It is about the same size as the small joint at the apex of the claw. The process which we have described, it will be seen, enlarges the passage and explains the difficulty which has so long puzzled both fishermen and naturalists; and it is so peculiar and entirely unlooked-for, as to be hardly a matter of surprise that it has so long escaped observation. The chief credit of this discovery we very willingly accord to Mr. S. M. Johnson, of the firm of Johnson & Young, of Boston, who have had about half a century's experience in the lobster business.

There is another peculiar condition in the lobster at the time of casting the shell, which is not mentioned in the books, so far as we know, and that is the presence on each side of the stomach of a small white substance, about as large as our five cent coin, which, when dry, hardens into a limestone. It is concavo-convex,

about one-eighth of an inch in thickness in the centre, thinning towards the edges, and formed of small spiculæ arranged transversely. These are only to be found in the lobster at the time of casting the shell, and are absorbed in the process of forming a new shell.[4]

Legislation on the Lobster Fishery. In the month of February of the present year, the two states on the Atlantic coast most largely interested in the lobster fishery, enacted laws for its regulation, somewhat different in their provisions but designed for the same purpose. In each state there were earnest advocates of a "close time," or prohibition, during what was supposed to be the "spawning season," and this was favored by high scientific authority. On the other hand it was held that there was no fixed spawning season, and that a close time was unnecessary and undesirable; and such a demonstration of fact and argument was made, that the purpose of a close time was abandoned so far as Massachusetts is concerned. In Maine, however, there were large and different interests to be reconciled, and the result was a law of quite different character from that adopted in Massachusetts, notwithstanding the fact that the interests of the two states in regard to the lobster fishery are believed to be identical, and notwithstanding a close time is sure to work hardship to the fishermen.

In Massachusetts, we may say, there is now a single provision of law which fixes the size of the lobster to be taken and kept or sold at any time at ten and a half inches in length, measuring from the head to the end of the tail (exclusive of the claws and feelers), and this regulation is for all seasons of the year. In the state of Maine, on the contrary, the law contains three principal provisions concerning the fishery, viz.: 1st, a close time "between the first day of August and the first day of November of each year;" 2d, regulating the size (ten and a half inches, as in Massachusetts), "from the said first day of November to the first day of April next following;" and 3d, free fishing "from the said first day of April to the said first day of August of each year." In the state of Connecticut, a law was passed, July, 1874, similar to the Massachusetts law, excepting in regard to the size,

[4] Similar substances, called white stones, are found under similar circumstances, in the land crab of the Bahama Islands.

which is fixed at ten (10) inches, measured in the manner above described.[5]

The effect of these laws is that in Massachusetts and Connecticut, the lobster fishery is protected by regulation for twelve months in the year; and in Maine, by prohibition and regulation, for eight months in the year. It is to be remarked that in the four months of free fishing on the coast of Maine (which period includes much of what some consider the spawning season), the canning factories are run at their utmost speed, and it is understood that everything in the shape of a lobster, which cannot be brought to market in Massachusetts, is disposed of by the fishermen at the factories — though it is claimed that at some of these a commendable discrimination is observed in regard to the size and character of the fish purchased.

Of course much may be said in relation to the wisdom and policy of these laws and the interests of the public and the fishermen in their operation and the practices under them. Undoubtedly some laws were necessary on the subject, not more perhaps for the protection of the fishery than for that of the public, and the interest of the Commonwealths in the lobster as food and as an article of commerce of very considerable importance. The experience of a year or two will enable all parties to decide what further legislation may be necessary, and an effort will doubtless be made for the adoption of uniform regulations in all the states interested.

NOTE ON THE GESTATION OF THE "LITTLE BROWN BAT," *Vespertilio subulatus* Say. By BURT G. WILDER, of Ithaca, N. Y.

THE only observations upon the gestation of bats, with which I am acquainted, are those of Owen, Comp. Anat. of Vertebrates III, 730. "The uterus of *Vespertilio emarginatus*, killed June 20, had a single fœtus, half an inch in length. A female of *V. noctula* produced, on the 23d of June, a young one with an umbilical cord two inches in length."

[5] It is known that there is a law in the British Provinces for the protection of the lobster fishery on that coast, but it appears not to have been enforced to any extent.

Upon these two observations is apparently based the following generalization. "Volant Insectivora, in relation to the exigencies of flight, are commonly uniparous."

The following instance indicates that one species forms a marked exception to the rule above stated.

About the middle of May, 1874, in Ithaca, N. Y., there were found in an attic, hanging near together, twenty-two females of the little brown bat, *Vespertilio subulatus* Say. No males or young were found. Each of the females contained *two* young bats, one in each horn of the uterus.

The bats were kept for three days without food or water, and probably were of less than their normal weight when killed; but the large size of the young is indicated by the following table of seventeen individuals. The weights are in grams and fractions thereof.

No.	Weight of Mother, entire.	Weight of uterus and contents.
1	7	2
2	7	2
3	7	1·5
4	8	2·1
5	8	2·5
6	6·5	2
7	6	1·5
8	6	1·5
9	7	2
10	8·5	3
11	7	3
12	7	2·5
13	8	3·5
14	9	2
15	6·5	2
16	7·5	3
17	7·5	2
Aggregate.	123·5	38·1

It will be seen that while in some as 3 and 14, the weight of the

uterus and fœtuses is only about $\frac{2}{10}$ of the whole, in others as 11 and 13, it is more than $\frac{4}{10}$. The ratio of the aggregate weights is about $\frac{3}{10}$.

Now even making allowance for the probable loss of weight from lack of food, the above table shows that bats are capable of sustaining in flight a considerable addition to their own weight. For although these individuals were not seen to fly, there can be little doubt of their ability to do so; and several authors have recorded observations of young bats found clinging to the parent. It would seem that in any calculations as to the power of muscle and length of wing which would be needed for human aërial locomotion, regard should be had to the above facts relating to the only truly volant mammals.

The large number of females in one place, and the total absence of the males suggest the idea that the latter may have been seeking food for the former. But no observations were made upon this point.

On the Male and Female Organs of the Sharks and Skates, with special reference to the use of the "claspers."

By F. W. Putnam, of Salem, and S. W. Garman of Cambridge, Mass.

ABSTRACT.

The authors of this paper called attention to the fact that while the so-called claspers of the sharks and skates had been generally regarded as having something to do with the copulation of the animals, very little direct information had been published on the subject. According to Agassiz, Aristotle had alluded to the subject, and had compared the copulation of the Selachians with that of dogs. Pennant, as if he knew their true character, in several instances makes mention of the organs by calling them the "double penis" and the "two genitals" of the male. It was, however, from the researches of Agassiz, made in 1856–7 but not

published, that the knowledge that the "claspers" were true copulatory organs became generally known. Agassiz afterwards made farther observations, and proved that these appendages to the ventral fins were capable of erection; still, while it was generally acknowledged that these singular appendages were intromittent organs, the necessity of the cutting edges and sharp points with which they are provided in the different genera was not understood. During the present season, unexpected light was thrown upon this peculiar part of the structure of the male organs by the dissections made by two of the lady pupils at the Anderson School of Natural History. Their dissections showed that a young shark (*Mustelus*) had the oviducts closed, while in those which were adult the oviducts were always widely opened at their outward extremity.

This observation led to farther examination on the part of the authors, and they found that certain modifications of structure in the virgin sharks corresponded to the peculiar structure of the male intromittent organs, and the only conclusion that could be drawn from the facts was that the "claspers," as they are generally called, were not only true intromittent organs, but that their peculiar and special armature of points and cutting edges was for the purpose of opening the closed oviducts of the virgin sharks in order to allow impregnation to take place.

On the Anderson School of Natural History at Penikese. By F. W. Putnam, of Salem, Mass.

ABSTRACT.

The author, premising his remarks with an allusion to the great importance of the school at Penikese in its bearings upon the methods of study in Natural History throughout the country, inasmuch as the majority of the pupils were teachers in schools and colleges all over the land, gave an account of the origin of the school from its first inception by Prof. Shaler to the extended

plan as perfected by the late Professor Agassiz, assisted by the liberal donation of Mr. Anderson.

Allusion was made to the immense amount of additional labor which the school had led Prof. Agassiz to perform during the last year of his life, and the high character and success he had secured for it through his disinterested efforts in establishing the means by which teachers throughout the country could obtain practical instruction during their summer vacations, the only time of the year when they could avail themselves of such opportunities.

The author reviewed the trials and success of the first year of the school, and gave an account of the buildings and extraordinary facilities offered at a mere nominal expense to the pupils. He then gave an account of the plan of instruction adopted during the present year by Mr. Alexander Agassiz, the successor of his father as the Director of the school, and of his unceasing efforts in carrying on the work commenced by his honored father, as well as the difficulties with which he had to contend from the isolated position of the school. The author then alluded to the interest which advanced teachers and specialists had taken in the school, their desire to have its good work continued, and their labors in its behalf at a time when, though tired out by their own work of the year, they were yet ready to lend their aid in the development of the important trust left by Agassiz. The disadvantages of the location of the school so far from the main land, and the peculiar paucity of animal life at the particular spot where it was located were pointed out. The author gave a sketch of the method of study pursued at the school. The pupils were taught to depend on their individual powers of observation, directed by the teachers. They were then led, by demonstrations and lectures, to make extended and careful dissections and comparisons themselves, and thus they soon obtained sufficient knowledge of the more prominent points in the structure of the animals they were studying to enable them to make such comparisons and special examinations as to become original investigators, full of confidence and strength in their own powers. In this way the pupils would be able to return to their own classes with a strength before unknown to them, and be able fully to appreciate what was best in the books, which, from the generally overburdened condition of the teachers, they were forced to depend on in their instruction to their classes. In relation to the fact that

the teachers were overburdened, he called attention to a statement which several had made to him that they were expected to teach not only natural history in all its branches, but also, either astronomy, mathematics, history, or languages, etc., and this in large Academies, Normal Schools, and even "Colleges."

The author then alluded to the system of the coeducation of the sexes, which had been practically illustrated at the school, to the very decided good results which had been secured, and to the perfect courtesy and good feeling which had existed among all the students. His own belief in the importance of giving both sexes equal facilities for study had been greatly strengthened by the results he had noticed at Penikese, and it certainly had been demonstrated there that the ladies were in every way as thorough in their work and as capable in its performance as the men.

The author of this paper was led to make these remarks from the great importance which he felt would result in the advancement of science in America from the continued instruction to teachers as followed out in this school.

TITLES OF COMMUNICATIONS.

The following titles of papers read in Section B include those accepted by the committee for publication in full, or by abstract, but of which the authors have failed to send copy, as well as those which the committee decided should be printed by title only:

The Genera of Butterflies studied Historically. By Samuel H. Scudder, of Cambridge, Mass.

On the Present Distribution of Woodlands within the United States. By Wm. H. Brewer, of New Haven, Conn.

Further Contributions to Physiographic Geology. By Richard Owen, of Bloomington, Ind.

Further Observations on the Geology of Northwestern Massachusetts, with Special Reference to the Hoosac Range. By Sanborn Tenney, of Williamstown, Mass.

Botanical Observations. By Wm. H. Seaman, of Washington, D. C.

Geological Map of the United States and Territories, with Critical and Explanatory Descriptions. By C. H. Hitchcock, of Hanover, N. H., and Wm. P. Blake, of New Haven, Conn.

On the Habits and Transformations of *Canthon Hudsonius* (Forst.) — the Common "Tumble-dung." By Charles V. Riley, of St. Louis, Mo.

On the Larval Habits of the Cantharid Genera *Epicauta* and *Henous*. By Charles V. Riley, of St. Louis, Mo.

On the Origin of North American Unionidæ. By Edward S. Morse, of Salem, Mass.

On the Relations of Dentalium. By Edward S. Morse, of Salem, Mass.

On the Ascending Process of the Astragalus in Birds. By Edward S. Morse, of Salem, Mass.

ON THE CAVE FAUNA OF THE MIDDLE STATES. By A. S. PACKARD, Jr., of Salem, Mass.

ON THE COMPOSITION OF THE POTTERY OF THE MOUND-BUILDERS. By E. T. COX, of Indianapolis, Ind.

PROGRESS OF SCIENCE IN MARYLAND. By Mrs. ALMIRA LINCOLN PHELPS, of Baltimore, Md.

ON THE SIGNIFICANCE OF CLASSES AMONG VERTEBRATES. By THEO. GILL, of Washington, D. C.

ON THE CHARACTERS AND RELATIONS OF THE AMERICAN GENERA OF *Cervidæ*. By THEO. GILL, of Washington, D. C.

ON THE RELATIONS OF CERTAIN GENERA OF *Canidæ*. By THEO. GILL, of Washington, D. C.

ON SOME LOCALITIES OF CONTACT OF TRAP AND SANDSTONE IN THE CONNECTICUT VALLEY. By WM. N. RICE, of Middletown, Conn.

A LIST OF THE VERTEBRATE ANIMALS OF OUTAGAMIE CO., WISCONSIN, WITH NOTES, ETC. By DAVID S. JORDAN, of Indianapolis, Ind.

REMAINS OF AN ANCIENT EARTH-WORK IN MARBLEHEAD, MASSACHUSETTS. By J. J. H. GREGORY, of Marblehead, Mass.

NOTES ON SOME RARE AND INTERESTING CARICES OF NEW YORK. By GEO. VASEY, of Washington, D. C.

THE PHYSICAL AND GEOLOGICAL CHARACTERISTICS OF THE GREAT DISMAL SWAMP AND THE EASTERN COUNTIES OF VIRGINIA. By N. B. WEBSTER, of Norfolk, Va.

ON THE TRUE CHARACTER OF THE SO-CALLED EOZOON CANADENSE. By L. S. BURBANK, of Woburn, Mass.

SMALL SIZE OF THE BRAIN IN TERTIARY MAMMALS. By O. C. MARSH, of New Haven, Conn.

ANCIENT LAKE BASINS OF THE ROCKY MOUNTAINS. By O. C. MARSH, of New Haven, Conn.

THE WINGS OF PTERODACTYLS. By O. C. MARSH, of New Haven, Conn.

On the Mechanical Condition of the Pebbles in the Newport Conglomerate, and their supposed Flattening by Pressure. By Wm. B. Rogers, of Cambridge, Mass.

On the Thickness of the Virginia Tertiary, as Indicated by the Artesian Borings at Fortress Monroe. By Wm. B. Rogers, of Cambridge, Mass.

Notes on the Paleozoic Formations of South America. By O. A. Derby, of Ithaca, N. Y.

On the Classification of the Indian Languages of Mexico. By Porter C. Bliss, of Boston, Mass.

Observations in a Visit to the Cave of Cachuamilpa, Mexico. By Porter C. Bliss, of Boston, Mass.

An Ascent of the Volcano of Popocatapetl in Mexico. By Porter C. Bliss, of Boston, Mass.

On the Organic Change Produced in the Bee by the Different Conditions to which it is subjected in its Larval State. By Mrs. Sophie B. Herrick, of Baltimore, Md.

How does a Young Bird Peck out of the Shell? By J. W. P. Jenks, of Middleboro', Mass.

Notice of a Pair of Trap-door Spiders from South America. By Charles R. Dodge, of Washington, D. C.

Traces of Ancient Civilization in Mexico. By Porter C. Bliss, of Boston, Mass.

EXECUTIVE PROCEEDINGS

OF

THE HARTFORD MEETING.

HISTORY OF THE MEETING.

The Twenty-Third Meeting of the American Association for the Advancement of Science, held at Hartford, Connecticut, during the week beginning on Wednesday, August 12, 1874, and officially terminating on the following Wednesday forenoon, was in several respects one that will mark a special era in the history of the Association.

In attendance, it was one of the three largest meetings since their resumption in 1866, and though none of the late meetings have reached the size of those of 1856 and 1857, yet there has been such a steady increase in attendance during the past seven years as to indicate a widespread interest in the meetings which will probably lead to far greater gatherings than have yet been witnessed. With the natural growth of the country, the number of professional scientists and those who are interested in the progress of science becomes proportionably larger, and the Association is, as it was intended by its founders that it should be, the annual attractive power to bring all together for a week of scientific discussion and social enjoyment.

The constitutional trouble, which for so many years has often caused the meetings to assume the temporary character of a parliamentary body, has been finally disposed of, and the routine business which must necessarily be transacted will be reduced to its minimum by the present definition of the duties and responsibilities of officers and committees, while the happy and cordial manner with which members having widely different views on some of the proposed changes, came together with finally united action for the general good of the Association, is conclusive evidence of the hold which the society has upon its supporters and their firmness in securing the untrammelled execution of its great object. The most important points involved in the changes that were effected were so thoroughly discussed at the meeting that it is not necessary to allude to them in detail here, but I cannot pass without mention, one or two of the great gains the new Constitution secures to the Association. Under it the members will now have as a permanent body, forming part

of the Standing Committee, those who from their acknowledged high position in the several departments of science have been elected from time to time as presidents of the Association, and who have in past years so faithfully and creditably sustained its reputation. Another most important object is also secured by the election of the principal officers of the sections a year in advance, thus not only preventing the delay in organizing the sections, and the hasty election of officers, but, by the additional dignity that has been conferred, it secures the undivided attention of the incumbents to their responsible duties; and it is expected that the Vice Presidents, who now become the permanent presiding officers of the two sections, and the Permanent Chairman of such permanent subsections as may be organized, will give additional interest to the meetings by their special addresses. The provision which enables persons who are not specially engaged in scientific work to attend the meetings as members, with every right and privilege conveyed by the title, will unquestionably prove a most satisfactory arrangement to the very large number of persons in the country who are so deeply interested in the objects of the Association and whose presence is so much to be desired. The establishment of a permanent fund, derived from the payment of the commuted assessments, in the form of life membership, the income of which is to be used only in aid of original research, is a beginning which will in future years enable the Association to take a more active part in the encouragement of original research. To this end the legal power to hold personal and real estate, given by the accepted Act of Incorporation, will also be conducive to much that is desirable.

The cordial manner in which a large number of the citizens of Hartford took part in the Proceedings of the Association, and aided in pecuniary and social ways, was additional evidence of the respect and consideration in which the Association is held by the people.

By the efforts of the Local Committee with the assistance and consent of the state authorities, accommodation was provided for most of the offices and meeting rooms in the State House. The general sessions were held in the Hall of the General Assembly, which was also used for the meetings of Section A, while the meetings of Section B were held in the Senate Chamber opposite. The Chemical Section was accommodated in the Court Room on the lower floor of the State House. On the division of Section B into two subsections, the Biological portion remained in the Senate Chamber, while the Geological was forced to leave the building for want of room and hold its sessions in one of the Halls of the Connecticut Mutual Building on the opposite side of the street. The Standing Committee were well accommodated in a quiet room on the lower floor of the State House, and the Governor's office was used as a general reception room by the Local Committee and for the business headquarters of the Association.

Owing to the season of the year many of the citizens of Hartford were absent from their homes, and consequently there was not so much social interchange between the citizens and the members as has been the case

at former meetings. With very few exceptions, the members were quartered at the hotels which had reduced the tariff of prices.

The Local Committee had done what they could in the way of securing free return passes over the railroads, and in many instances the roads had responded in a willing and liberal manner, but the recent extended combination of the railroad companies against the pass system, in several instances made it impossible for the committee to secure return passes for those members who, coming from a great distance, most needed this very important reduction of the expenses of attending the meeting which has generally been so willingly granted. The efforts of the Local Committee in relation to social receptions and interesting and scientific excursions were decidedly successful, and received the hearty approbation of all the members, who could but feel grateful for this kindness on the part of the committee.

At 4 P. M. on Thursday about fifty members accepted the invitation to visit Trinity College, and afterwards the quarries at Rocky Hill. In the evening the members were received by Dr. H. P. Stearns at the Retreat for the Insane and passed several hours in examining the buildings and the arrangements made for the comfort and restoration to health of the patients at the Retreat. The delightful grounds and the music on the lawn added much to this pleasant social gathering of the members and many citizens.

On Friday evening a visit was made to Colt's Armory where the capabilities of the terrible engine of destruction, the Gatling gun, was exhibited to the full satisfaction of the members of the Association.

Saturday was entirely given up by the Association to what proved to be the most delightful and interesting of excursions, a trip down the Connecticut River to its mouth and out beyond the breakwater and return. For this excursion the "City of Hartford," one of the largest steamers running between Hartford and New York, was selected, and the several hundred citizens and members were very comfortably provided for. The day proved to be most delightful, and the return trip in the evening added much to the charm of the beautiful and varied scenery along the river.

On Monday evening an excursion was made to Tariffville for the special benefit of the geologists who were much interested in the peculiar features of the trap rocks there observed.

Tuesday evening was taken for a visit to the unique and beautiful model village of Cheneyville where the manufacture of silk was shown from the preparation of the cocoons to the completed fabric. A stroll over this village, which is one grand combination of lawn and flower garden, unbroken by fences or boundary lines, with its well-kept gravel walks leading from cottage to cottage and equally neat roads, showed that the proprietors had succeeded in making a manufacturing centre a most delightful place. The contrast between its neatness and thrift and that of manufacturing towns with which we are more familiar argues well for the philanthropy here exhibited.

During the week several small parties of members also visited various places of interest in Hartford and vicinity under the guidance of members of the Local Committee.

After the adjournment on Wednesday a large party of members, accompanied by many citizens, made an excursion to the Sandstone quarries at Portland and to the Wesleyan University where they were hospitably entertained by the citizens of Middletown.

On the following day many of the members of the Association, who had returned to Hartford after the visit to the Portland quarries, were conducted on an excursion through the most beautiful portion of the State of Connecticut, over the Connecticut and Western Railroad to Lakeville and the Iron mines of Salisbury.

A large number of entomologists were present at the meeting and, in pursuance of the plan proposed at Portland, they organized as a special body under the title of the Entomological Club of the A. A. A. S. This club held several meetings and elected the following officers:—J. L. Le Conte, *President;* S. H. Scudder, *Vice-President;* C. V. Riley, *Secretary.* The following rules adopted by the club and the resolutions passed will convey the objects which it has in view.

TITLE. The name of the Association shall be "The Entomological Club of the American Association for the Advancement of Science."

OBJECTS. The annual reunion of the Entomologists of America, the advancement of entomology, and the consideration of all general questions relating to the science that may from time to time arise.

MEMBERSHIP. All members of the American Association for the Advancement of Science specially interested in Entomology shall, *ipso facto*, be members of the club.

OFFICERS. The officers of the club shall be a President, a Vice-president, and a Secretary, to be elected annually by vote of the members.

DUTIES OF THE OFFICERS. The President, or in his absence the Vice-president, shall preside at all meetings; the Secretary shall perform all the usual duties of a recording and corresponding secretary.

MEETINGS. A meeting shall be held in each year at the place of meeting appointed by the American Association for the Advancement of Science; it shall commence at 2.30 p. m. on the day before the meeting of the American Association for the Advancement of Science, and be continued throughout that evening; further meetings may be held as time will permit during the week following.

Resolved, That the members of the American Entomological Society and the Entomological Society of Ontario, together with all other persons interested in entomological science, be cordially invited to attend and take part in the proceedings.

Resolved, That the Secretary be requested to publish notices of the meeting in such periodicals as are devoted to natural history, and especially in those devoted to entomology, as are published on the continent; and further, that the members be requested to bring with them at the annual reunions specimens for exchange and exhibition, and especially types of species that they may have described during the year.

The action taken by the Entomologists and the formation of a Permanent Subsection of Chemistry, which was also accomplished at the meeting, are so fully in accordance with the objects of the Association in

bringing together scientists in all departments, that this expression on the part of two special branches can only be regarded as most favorable towards the annual centralization of scientific thought in the country during the Association week, and it cannot be long before the American Association will draw within its folds, like the British Association, many special organizations now existing, which would be benefited by a union with the Association, and working for one common end would thus still more greatly aid in the Advancement of Science in America.

The first general session of the twenty-third meeting was called to order at 10 o'clock on Wednesday morning, by Prof. Joseph Lovering who, by a few appropriate remarks, resigned the chair to his successor, President LeConte.

In the afternoon the Association was formerly welcomed by the Hon. Henry C. Robinson, as chairman of the local committee, in behalf of the citizens of Hartford.

On Friday night the Address of the retiring President, Professor Lovering, was delivered before a large audience in the General Assembly Hall of the State House.

Section A was organized on Wednesday and continued its sessions until the Tuesday evening following, giving off a subsection of Chemistry on Monday which also adjourned on Tuesday, after making preparations for its permanent organization at future meetings.

Section B was organized on Wednesday, and subdivided into Biology and Geology on Monday. The subsections were continued until Tuesday evening. After the adjournment of the general meeting on Wednesday, an informal meeting of the section was held for the reading of papers that had been accepted, but from some mismanagement had not been duly brought before the section.

The twenty-third meeting was adjourned on Wednesday evening, August 19, and the twenty-fourth meeting will be held in Detroit, Michigan, in accordance with the vote accepting the very cordial invitations extended by the Governor of the State, the Mayor of the City, and the Detroit Scientific Association. The meeting will begin on August 11, 1875 (the *second* Wednesday), at 10 o'clock A. M., and a meeting of the Standing Committee will be held in Detroit on the day preceding.

The total number of members who have paid their assessment for the Hartford meeting, to this date, is five hundred and eighty-nine; two hundred and twenty-four members signed the register at the meeting; one hundred and eighteen new members were elected, and of this number all but ten have since accepted their membership and paid their fees; two have declined membership and eight have not yet answered to their notifications.

The name of Mrs. Thompson was placed on the list as the first patron of the Association in accordance with the recommendation of the Standing Committee.

Three members have paid the sums admitting them to life-membership.

Up to the time of these pages going to press one hundred and fifty

two members have had their names enrolled as fellows, in accordance with the article in the new constitution in relation to fellows.

Eight subscriptions were received at the meeting for the volume of Hartford Proceedings.

One hundred and sixty-five papers were entered on the list of titles received. Of these sixty-six were referred to section A, and sixty-nine to section B, including the subsections of each; seven were withdrawn by their authors; thirteen were declined by the committee, and ten were refused consideration as their authors had not complied with the rule requiring abstracts to be furnished or the papers themselves to be submitted to the Standing Committee. Of the papers read ninety-nine were accepted for publication either in full or in abstract, but as the Standing Committee recommended that the volume should not exceed four hundred and fifty printed pages, it was deemed advisable not to call on the authors for their papers and only those which were sent in by their authors in accordance with the rules of the Association have been put in print. As every effort will be made to furnish the next volume of the Proceedings to members at an early date, and as the new constitution requires that the papers to be printed must be in the hands of the Permanent Secretary within one month after the adjournment of the meeting, the Permanent Secretary earnestly requests authors to have their papers prepared so as to give them to him at the meeting. In this way the printing of the volume can be started at the moment of acceptance of the papers by the Standing Committee, and in this way alone can the volume be brought out with promptness. It would also save much time if authors would have fuller abstracts of their papers prepared for the use of the Standing Committee than is now often done. Not only are papers apt to be misjudged from the short abstracts submitted, but the substance of a paper can often be printed with full justice to the author in the form of an extended abstract when the paper itself might be refused simply from the want of room in the volume. As these suggestions are made to members after considerable experience on the part of the Permanent Secretary, and wholly from a desire to obtain their hearty cooperation in securing a prompt issue of the next volume of the Proceedings, he hopes that they will be carefully considered and that the aid here asked will be granted.

The following report of the General Secretary contains information such as has heretofore generally been incorporated in the history of the meeting by the Permanent Secretary.

F. W. Putnam,

Permanent Secretary.

REPORT OF THE GENERAL SECRETARY.

THE ASSOCIATION assembled at the State House, at 10 A.M., on Wednesday, August 12, 1874, and was called to order by the retiring President, Prof. JOSEPH LOVERING, who, in a brief speech, introduced the President elect, Dr. JOHN L. LECONTE. The customary prayer was then offered by Rev. Dr. RIDDLE of Hartford, and the president delivered the opening address as follows:

FELLOW MEMBERS OF THE ASSOCIATION: A year has passed since we last met to exchange ideas, to communicate facts, and by the charms of personal contact, to strengthen each other for the continuance of our pleasing labors in the cause of science. The year has not elapsed without salutary signs, which make us hopeful both for the future progress of human knowledge, and for that, which, though not immediately in the direction of the work of the Association, is equally dear to us as men, having in view the happiness of our fellow-beings.

You will pardon me, if I detain you for a few moments to mention four events which in my judgment may be properly brought to your attention, though many others are equally noteworthy.

A heavenly visitor has come to us from the distant regions of space, by the careful study of which, with instruments unknown to former observers, the chemistry of those remote parts of the universe may be brought within human cognizance. In its progress through the abyss of space, the Solar System has annexed another wisp of Primeval Chaos, and attached it, temporarily or permanently, I know not which, but you will shortly learn, to our portion of the Cosmos; whether it appear again to human eyes or not, it will live in our memories, and in the history of science as Coggia's Comet.

For the first time the spectroscope has been directed upon such a body of respectable size. From the examination we may expect the happiest results. If the investigation is not altogether complete, data will be obtained which will make it perfect with future opportunities.

In Section A you will doubtless be told much about the chemical composition of the comet, not only qualitatively, but perhaps with a rough quantitative approximation to truth. You may indeed, with a pardonable exercise of human vanity, leave these walls inspired with the belief, that had you the power, you possess fully the knowledge, under favorable circumstances, to make one of these comets, the simplest of the bodies of the universe: the protoplasm, if I may so express myself, of the inorganic world.

Nevertheless, that is not what I wish to arrive at—

As the telescope, the first of the great instruments of precision given to modern science, by enabling astronomers accurately to observe, calculate and predict the orbits of comets, thus disarmed them of the superstitious terrors with which they were previously surrounded, so does the

spectroscope, the last and most wonderful instrument of precision, by its power destroy the remnant of fear still attaching to these occasional compulsory visitors, by showing that even if the largest of them were to strike the earth, no possible harm could result. May I go yet farther, and hazard a conjecture, that the study of comets by the spectroscope will eventually confirm the thought that our planet has been much benefited by occasional collisions and absorptions: that some of the rarer chemical elements, found only in superficial strata, may have been added to our earth by these means, and owe their widely extended diffusion to the same causes.

Thus has it come to pass, that these celestial portents, formerly the presagers of woe, have been changed by the beneficent magic of science into bearers of good tidings, and of cheerful hopes. Tidings that the laws we have observed and formulated on our earth are coextensive with the material universe: hopes that the knowledge of man may be yet increased in directions and beyond limits which the wildest imagination of half a generation ago would not have dared to indicate.

The second event to which I would call your attention, like the remaining two, is purely human.

The Anderson School at Penikese, for instruction in zoology, has been established on a basis, the success of which gives evidence of permanence. Amidst the grief we feel for the loss of our beloved friend and guide in science, who, by the aid of the most generous patron after whom it is named, founded the school, we must all rejoice that though his noble presence has disappeared from among us, the spirit of Agassiz yet lives and moves. The labor of his life survives him in more than books and collections; it survives as an active organization, which will give to hundreds, nay, to thousands, the benefit of his methods, and the power of profiting by the best instructions in zoology; and it will prepare them according to their desires, for teaching, or for the higher and more laborious task of investigating.

The third event is local, but is important. Because, moved by a generous rivalry, other cities will hasten to imitate Philadelphia in the establishment of a Zoological Garden, and also because the sympathy with which the project has been received showed an appreciation of the beauties of science, and a growing interest in its popularization, which must be very gratifying to every naturalist.

The last event in this retrospect, for which I will claim your ear, is one which promises much for that which has in all ages been the highest effort of the highest minds—the alleviation of human suffering.

You will smile when I mention it, it is so perfectly simple, and when viewed from a scientific standpoint so entirely behind the time.

It is only this: the introduction into the United States of *civil hospitals* built on the plan which is known in every other civilized nation as the American System. Although familiar to the great bulk of our citizens during the sad experiences of the war, and although it has vitiated by its successful results all the former statistics of medical and surgical treat-

ment, this construction has been neglected in the land in which it originated. At last, however, under wise scientific counsel, a wealthy and influential religious sect (the Presbyterian), also in Philadelphia, has authorized and commenced the construction of a hospital of four hundred beds upon this plan.

This is not the place, nor the time, to inquire into the causes which have for so many years prevented the mass of civilians from profiting by the experiences of the largest and most efficient medical corps that has ever been in the employment of any government.

(It must be here observed, parenthetically, that the greater efficiency of the medical corps, both regular and volunteer, was owing probably not to superiority of professional attainments over our brethren in foreign service, but to the fact that in the army of the United States, the Medical Staff has possessed military power, if not command, to a degree unknown in any foreign army.)

It is worthy of remark, that now, tried and approved by the highest foreign authorities, our own invention has come back to us, without essential improvement. We may therefore expect that the results obtained by the excellent students of sanitary science, who were in the service of the government (among whom, I am happy to remind you, the General Secretary of this meeting was one of the most eminent), these results, I say, will now be appreciated at their proper value, and their benefits experienced by patients in civil life.

Should this meeting be the occasion, and these desultory remarks the humble means of inducing the men who liberally and philanthropically found and endow hospitals, and our own municipal authorities, who without stint tax the citizens for the same purpose, to insist that the structures shall be planned with reference to sanitary principles rather than architectural display, the time that I have detained you upon this subject will not have been wasted. Low, inexpensive, ridge-ventilated pavilions, well raised from the ground, and with inner walls which may be readily renewed when poisoned with effluvia, will take the place of the heavy stone and brick piles in which so many human victims to Æsculapius are annually sacrificed: every small town will have its own well arranged hospital, and a tardy, even yet perhaps tacit, justice will be rendered to the labors of those medical officers who devised the American System.

With the specific facts, indicating during the past year a steady progress in the branches of science which you respectively cultivate, you are already familiar through the scientific journals, and memoirs of learned societies: it is unnecessary for me to allude to them.

The four examples, upon which I have dwelt, have been mentioned in order to show, not the immediate objects, but the final results of true Catholic science. They are these: the suppression of superstition; the enlargement of education; the cultivation of higher tastes; and the alleviation of suffering. Thus does the pursuit of science, proceeding at first from an abstract love of truth, link together in one harmonious bond, all that is noblest in human endeavor, and benefit equally the moral, the intellectual, the æsthetic and the physical condition of man.

Many members of the Association who were with us on former occasions, and have contributed much to the value of our Proceedings, are absent on long, though fortunately not perilous, voyages, to establish stations for the observation of the transit of Venus. We miss them greatly, but our loss is science's gain. They will return to us next year, undiminished we hope in number, unimpaired in zeal, and heavily laden with weighty truths which they will communicate to us at fitting opportunities.

Our regret at the absence of many astronomers, who are thus on detached service, is alleviated by the joy with which we welcome to the meeting our ex-president, Dr. B. A. Gould. During the four years of his separation from us, he has been worthily engaged in establishing and conducting an astronomical observatory in a distant part of the Southern hemisphere. We will all rejoice that the enlightened policy of our sister Republic, the Argentine Confederation, has appreciated the importance of such an institution, and that the responsible duty of organizing it and directing its operations has been given to a colleague so well fitted by his previous labors in science to do honor to our nation in accomplishing the work which prompted him to the original undertaking.

Ladies and gentlemen, I have done. I have already occupied more of your time than I intended. I must remember that I have been placed in this honorable position, not to express my opinions, but to execute your commands. Every moment that detains you delays the pleasure of hearing those who will address you with eloquence and power upon the subjects which we have come together to discuss.

The General Secretary, Dr. A. C. Hamlin, then reported the list of papers acted upon by the Standing Committee and referred respectively to Sections A and B. The names of those proposed as members were read and the persons were elected. The arrangements made by the Local and Standing Committees for the session and for the entertainment of the members were reported and acted upon.

Among the invitations received were letters from Dr. H. P. Stearns to visit the Retreat for the Insane; from Gen. Franklin to visit Colt's Armory and an exhibition of the Gatling gun; His Excellency Governor Bagley of Michigan, His Honor Mayor Moffat of Detroit, and from the Scientific Association of that city, inviting the Association to hold its next meeting at Detroit. These several invitations were referred to the Standing Committee.

The Association then elected six members to the Standing Committee, viz.: Professors Wm. B. Rogers, J. E. Hilgard, G. F. Barker, Asa Gray and E. S. Morse.

The Permanent Secretary, Mr. F. W. Putnam, announced the decease of five members during the year:—Louis Agassiz, Asa Whitney, M. Barrett, Edwin F. Adams and Minshall Painter.

On motion of Prof. J. L. Smith, the Standing Committee was authorized to provide for obituary notices of the late Presidents of the Association.

Mr. W. W. WHEILDON reported upon the Act of Incorporation which had been obtained from the Legislature of the State of Massachusetts. After some debate the Act was accepted.

The recommendation of the Standing Committee that the morning session commence at 10 A. M. and close at 12.30, and that the afternoon Session begin at 2.30 P. M. was agreed to.

The report of the Special Committee on the Constitution was taken up and read by sections by the General Secretary.

Mr. WHEILDON also reported a preamble to the Constitution which was adopted.* After much discussion and many changes in the phraseology of the constitution the subject was postponed to a special meeting to be held at 5 P. M., and the Association adjourned at 12.30.

The afternoon session opened at 2.30 o'clock, when Hon. HENRY C. ROBINSON, as chairman of the local committee, in behalf of the citizens of Hartford, delivered the following address of welcome to the members of the Association.

MR. PRESIDENT AND GENTLEMEN OF THE ASSOCIATION:—

It is my duty, in behalf of the local committee, to welcome you to our city, and to offer to you such accommodations and conveniences as may serve you in your sessions, and such measures of hospitality as we may. We regret that your gathering must be at a season when our homes are sensibly deserted, and when seclusion is more luxurious than is sociality. But though our demonstrations must be less marked, our greeting to you is as sincere as if made in the young green of June or the scarlet of October.

With the purpose of your society our community has complete sympathy, and its results have awakened in us our share of patriotic pride. Your Association seems somehow like a patent office for scientific discovery, only that the invention is covered by no monopoly to the inventor, but its blessings are scattered on the wings of wires and types to the limits of civilization. In the republic of science the rewards are the joys of discovery, and in that contribution to the commonwealth of humanity, which each new revelation of truth brings in, in greater or less degree. As loyal, too, to the nineteenth century in its best thoughts, we sympathize with you in your partiality to the real sciences. We gladly utilize your successes in ascertaining and applying those positive manifestations in the universe, whose study has already wrested the supremacy in the world of thinkers from speculative philosophy. We would not underrate the value of metaphysical speculations, when looked upon as intellectual gymnastics for their students.

Nor should we forget that they have wrought bravely in developing constitution-makers and hard-headed reasoners in things political, ethical and religious. And yet, it requires no searching insight to see that speculative philosophy has been forced to step down from its throne so long

* In the final revision the preamble was omitted.

held in the world's worship, while positive truth, dug out, and hammered out, and melted out, and spied and heard, and felt in the cosmos of things physical, mental and spiritual, has taken new dignities in the estimates of scholarship, and new respect in the instincts of the people. "Harper's Magazine" prefaces its love stories with illustrated accounts of travel and discovery, and sandwiches the latest news in chemistry and astronomy between its verses and its funny stories, while the circle of Hegelian students is narrowing, in geometrical progression descending.

Since your last gathering, the world has mourned with you over the ashes of your scientist most eminent, and man most noble and fascinating. Many eyes were dimmed with tears when his gentle eye was dimmed in death. Fearless in investigation, honest in deduction, reverent in spirit, science herself has turned mourner for him, even as art mourns in marble at the tomb of Canova. I have said that the true disciple in science finds his chief rewards in his discovery itself, and in its blessing to the world. But it is not unpleasant to notice that more and more the truth-discoverers are getting for themselves a personal immortality in the love and respect of mankind. Twice within a few weeks have we observed that others than men of blood and of the sword are remembered in the notches of centuries—once in Italy by the grave of a patriot and poet, and once in Pennsylvania beside the dust of a chemist.

Gentlemen, an association like yours whose range of study is as wide as the universe, whose legend is truth, whose consecrations are pure, and broad, and fair, whose ends are knowledge and discovery, must be most welcome to any community which is alive to the welfare of the race. Most of all is this true when you come in such ways of intellectual friction and social sparkle as the personal association and interchange of thought between such gentlemen as I have the honor to receive, eminent in specialties and in general learning, must develop. Your annual sessions, rich as the ingathering of harvests, are wisely arranged to cover with blessings our whole nation by their migrations from city to city.

I shall enter into no extended rehearsal of the contributions which this particular community has made to science. In skilled mechanism, especially in the manufacture of firearms, and steam engines, and sewing machines, and drop forgings, and feed heaters, and eyelets, and axes, and cutlery, our community is prominent. Nor are we less successful in the manufacture of silks and carpets, or in the revelations of strength and beauty from the heart of freestone and marble and granite. We fancy that we can show to you the best appointed manufactory in the country and the most charming church edifice in the class of smaller churches. We believe that American character and industry have produced no better model of a village than the Cheney village in Manchester, seven miles away, where capital and labor complement each other without jar or friction.

We are glad to boast that the governments of the European continent, and even of the remote East, are often contractors for our mechanical skill. We are proud, too, of our methods of general education for chil-

dren and youth, and of our treatment of the insane, and of the deaf and dumb, of the text-books for schools and academies which have been edited and published here, and of our composers of music and expounders of its science, and of many other special things which I need not tell, but by which our city and community have added their full share to the treasury of science.

But I may be pardoned for mentioning to you in our pride, one yet living with us, and late be his call to rest! who has wrought most eminently and in all honesty and sincerity, and with the majesty of his great imagination, and the extent of his seership, and the power of his words, for the harmony of science and religion. He believes that the Light is true "which lightens every man," scientist and all, and fears for the honor of Incarnate Truth in the presence of no phenomenon of scientific discovery. And I may be pardoned, too, for reminding you that this assembly is even now sitting within a short stone's throw of the office formerly occupied by one who has already taken rank with the great inventors and benefactors. A modest, quiet, busy man, observing, inquiring, investigating, whose profession of dental surgery, brought him into contact with human suffering of greater or less intensity, witnessed one day an already well known scientific process. Its application to his own profession was perhaps the natural transfer which an investigator makes of things which he sees, new and old, to his specialty; perhaps it was an inspiration. We need not philosophize about it. The application was made, and Horace T. Wells gave to the world anæsthesia. In this learned presence I will attempt no theory or guess at its results of to-day and of the future. Such reliefs from pain, such deadening of quivering nerves, such new opportunities for skill given to surgery, from civilization's end to civilization's end; and from that hour of invention to the last hour of time, defy the computations of mathematics and the descriptions of rhetoric.

Members of the Association, in behalf of the citizens of Hartford represented in your local committee, I give to you a cordial welcome.

President LeConte responded to Mr. Robinson in the following words.

Mr. Chairman:—The large body of learned men, now present before you, have assembled from all parts of our country, and from the neighboring dominion, in accordance with the hospitable invitation received at the Portland meeting, a year ago.

For that invitation, and for the cordial greeting with which you have met the Association, I, its unworthy representative, offer you our most sincere thanks. Such a high appreciation of the noble objects of our meeting is most worthy of the reputation for learning, hospitality and cultivation, which this community, in which you have held the highest position, so deservedly enjoys.

It is but a short time since you welcomed heartily and entertained liberally, a younger sister of this Association, the American Philological Association. The members of that learned body deal with some of the most difficult problems submitted to human thought; the origin, history, structure and development of language, without which all progress would

be impossible. The methods of investigation are in part the same, and the qualities of mind for pursuing them somewhat similar, but our scope is far wider and grander: the origin, history, structure, and (humbly and imperfectly, it is true) the future development of the Cosmos, and of each individual part of it.

We have assembled, from near and from far, to give to each other the results of our year's questioning of the great powers, which rule, with apparently inflexible laws, the material universe in its greatest phenomena, and in its smallest details.

And we have also met to enjoy, in the few hours of leisure, which necessarily must intervene between our sessions, the generous provision for our entertainment so amply set forth in the programme of the Local Committee. Essential means are they, not only for forming the acquaintance of our hospitable hosts, but for the better knowledge of our fellow members. On such occasions, friendships and scientific sympathies are established, which are far from ending with the meeting, and which often worthily supplement the work done in our protracted sessions.

Like good school boys, for all men of science are pupils in the great school of nature, we love work, and also, at fitting times, we love play; and the relaxation which many of our number, overworked and underpaid during a great part of the year, obtain at these meetings, does much to develop that endurance of brain, which enables them occasionally to give to science the few hours of what would be the leisure of persons less devoted to intellectual progress.

Therefore we thank you for these opportunities for social pleasure which you offer us; doubt not, they will be gratefully accepted and heartily enjoyed.

I have already mentioned incidentally one of the purposes of our assemblage in these annual meetings. The objects of the Association have been so often declared, that it may appear superfluous to notice them again. Yet since each year we meet in presence of a different community, and each year the reports of our meetings are spread before a larger class of readers in the general public, a brief announcement of the endeavors and intentions of the Association would not seem to be out of place.

These learned men come thus together with no trivial or small motive. If I may be allowed on such an occasion to use a familiar expression, they have no axe to grind; save only the axe of science,—the keen edge of accurate observation, with which to hew a path through the dense thickets of physical difficulties and human errors, which impede intellectual and moral advancement.

They assemble not with personal objects in view; neither to attempt to rule the policy of the nation, nor to seek office, nor to put up the price of capital or labor, not even to extend their reputation as men of science, for of this they are already secure, in proportion to their merits, in the esteem of their colleagues; in fine, they meet for no selfish purpose whatsoever.

They have met for high and sacred duties. Mindful of the dignity of

their calling, as seekers after truth, — physical truth, which the evidence of the senses cannot deny, nor the reasoning power of the brain reject,— they are generally protected against the sources of error and temptation, which assail men of less exalted pursuits. Personal vanities, passionate claims for priority of observation, squabbles about nomenclature affecting individual interests have no proper place in our midst. And, indeed, such disputes, such personalities are in science, quite unnecessary. As a dear friend, alas! gone from among us, so that we can profit no longer by his words, but only by his good record, once said to me: Science is the only true Republic; in it, every one has full liberty to do what he pleases, and what he does is valued at exactly what it is worth.

It has been said by my predecessor in office, that we do not concern ourselves with the utility of the truths which are communicated at these meetings. In one sense this is correct, for to the lover and seeker after truth, that manifestation of divinity is more beautiful and more elevating than any of what are popularly called 'practical applications' of the facts observed.

In another sense, the sentiment is inexact; for the object of all rational human effort is to elevate the individual and to improve the race.

The first and highest utility of the truths we seek is to increase the intellectual grasp, enlarge the observation and control of surrounding objects and purify the moral tone of ourselves and our brethren. The outflow of physical comforts, and material wealth which sooner or later come to inventors, as the result of our labors, are to us secondary considerations which we watch with interest, and which none hail with more satisfaction than the investigators who have rendered the invention possible.

I might go farther, Mr. Chairman, and endeavor to show how scientific men, who, I suspect, in this day and land, are contemned for not being practical, are in reality the most practical of all men. They render by well directed effort, what is impossible one year, probable the next, and possible soon afterwards. It is upon these impossibilities, made possible by science, that the so-called practical men proceed to experiment and build their fortunes.

This thesis is to me a fascinating one upon which I would gladly expand, but passing time admonishes me to be brief. Other speakers, more endowed by nature and experience in holding the attention of an audience are prepared to follow me in expressing the thanks of the Association for the distinguished courtesy that has been shown us.

The wonderful beauty of your valley has been praised in far nobler language than any at my command. The scientific treasures of your neighborhood are well known to many of us, who have never had the pleasure of seeing them. Your institution of learning has a well-merited reputation, which extends beyond the most distant homes of your guests. All these we are glad to see, and to appreciate better by fuller acquaintance.

But there is one point in which, as an American, and speaking for Americans, I must say that your beautiful city possesses a still higher interest. It is one of the nuclei of liberty, from the coalescence of which

at a later period, was formed at Philadelphia the embryo of this great nation.

Here was performed one of the earliest acts of resistance to that mistaken policy pursued towards the North American Colonies of Great Britain, which afterwards resulted in their complete separation: without bloodshed it was, but brave, and noble, and prudent withal. The names of the men who saved the Charter from illegal seizure are known to every school boy in the land, and the picture of the oak, so highly honored as its depositary, has been seen by every child; and will be by those to come. However high, or however low may be the destinies which God has prepared for the nation, it will ever be remembered that there were patriots in those days, and that some of them lived in Hartford.

On motion of Prof. ROGERS of Philadelphia, the consideration of the Constitution was taken up, but after a long discussion the question was postponed until the next day.

Immediately after the adjournment of the General Session the two Sections A and B were organized and reported as follows:

SECTION A was called to order by Prof. LYMAN, and Prof. SIMON NEWCOMB was chosen as Chairman with Prof. A. R. LEEDS as Secretary. J. L. SMITH, J. H. COFFIN, and H. C. BOLTON were elected as the Sectional Committee.

SECTION B was called to order by Dr. J. L. LECONTE, and Mr. S. H. SCUDDER was chosen Chairman with Dr. THEO. GILL as Secretary. S. F. BAIRD, E. T. COX, and T. STERRY HUNT were elected as the Sectional Committee.

The second day's Session commenced at 10 A. M., on Thursday. The General Secretary read the names of proposed members and they were duly elected. The additional assignment of papers to the Sections was also announced.

An invitation from the MAYOR and citizens of Williamsport, Pa., to hold the next Annual Meeting in that city, also invitations to visit Trinity College and the grounds of Mrs. COLT, were read and referred to the Standing Committee.

The request of the chemists for the formation of a Permanent Subsection of Chemistry was referred to the Standing Committee.

The discussion of the Constitution was again resumed, and after many alterations the whole subject was referred back to the Standing Committee for a revision in accordance with the expressed desires of the Association.

The General Session then adjourned, and the Sections assembled for the reading of papers.

The Sections reported, as follows, to the General Secretary:—In SECTION A, Messrs. TROWBRIDGE, ELLIOT, LANGLEY and JOHNSON were elected members of the Nominating Committee. In SECTION B, Messrs. KERR, WHEATLAND and BAIRD were elected members of the Nominating Committee.

The third day's Session commenced at 10 A. M. on Friday, and after the election of the proposed members, and the reading of the list of additional papers assigned to the Sections, the Association adjourned to meet as Sections.

At 8 P. M., the address of the retiring President, Prof. JOSEPH LOVERING was delivered before the Association, in general session, at the State House.

The fourth day of the meeting was devoted to the excursion down the Connecticut River.

The fifth day's Session commenced on Monday at 10 A. M. The proposed members were elected and the assignment of additional papers was read. Invitations to visit the Wesleyan University, the Granite Polishing Works, the Sand Blast Works, and the State Prison, were read and referred to the Standing Committee.

The preparation of obituary memoirs of deceased presidents was announced as having been assigned as follows:—to Prof. SWALLOW, that of the late Col. Foster; Prof. GEO. THURBER, that of the late Prof. Torrey; and Prof. GUYOT, that of the late Prof. Agassiz.

Prof. JOSEPH HENRY was chosen to fill the vacancy in the Standing Committee caused by the absence of Prof. W. B. ROGERS.

Prof. J. E. HILGARD reported progress of the committee on weights and measures and the committee was continued.

The Committee to memorialize the Legislature of New York for survey of Niagara Falls reported progress, and was continued.

The Committee to report on the best methods of conducting geological surveys failed to report and was continued.

The Committee to memorialize Congress in relation to a geological map of the United States reported progress and was continued.

The Committee on the Elizabeth Thompson fund reported that it had accepted for publication the memoir of S. H. Scudder on Fossil Butterflies.

The Committee on nomenclature reported progress and was continued.

The Committee on methods of studying science in common schools failed to report and was discharged.

The Committee on the preservation of forests reported progress; also the probability of the passage of a law by Congress this winter, and the committee was continued.

At the close of the reading of these reports Prof. TUTTLE offered the following:

"*Resolved*, That the preamble and resolutions relating to the subject of scientific education, adopted at the 22nd meeting of the Association, whose committee has just been discharged, be re-adopted and a new committee be appointed."

The resolution was referred to the Standing Committee.

The Association instructed the Permanent Secretary in making out the lists of past officers, to include the names of those in office when the organization was known as the *Association of American Geologists and Naturalists*.

The revised constitution was again presented by the Standing Committee, and read section by section by the General Secretary, and was unanimously adopted.*

Prof. BARKER offered a resolution which was amended by Prof. LYMAN so as to read as follows:

Resolved, That the revised constitution go into effect at the opening of the next meeting; but in so far as may be necessary in making the elections for that meeting, that it take effect immediately: Adopted. Adjourned into sections.

The sixth day's Session commenced on Tuesday, at 10 A. M. After the reading of the names of proposed members who were duly elected, and the list of newly assigned papers, Prof. BARNARD made an additional report in behalf of the Committee on uniformity of weights, measures and coinage.

The Association then proceeded to the election of officers for the next meeting and the following were chosen.

J. E. HILGARD, *President.*

H. A. NEWTON, *Vice President, for Sec.* A.

J. W. DAWSON, *Vice President, for Sec.* B.

F. W. PUTNAM, *Permanent Secretary* for five years.

S. H. SCUDDER, *General Secretary.*

S. P. LANGLEY, *Secretary of Sec.* A.

N. S. SHALER, *Secretary of Sec.* B.

WM. S. VAUX, *Treasurer.*

The following resolutions from the Standing Committee were read and adopted:—

Resolved, That this Association is impressed with the advantage to science which would be derived from the taking of a centennial census of the United States upon the 1st of June, 1875, the results of which should be ready for publication in the following year, as a feature of the general celebration of the completion of our first century of national existence.

Resolved, That the President and General Secretary of this Association are authorized and requested to communicate these resolutions officially to the President of the United States, to the Secretary of the Interior, and to both Houses of Congress, calling attention to the importance of favorable action thereupon.

Also the following which was adopted:—

Resolved, That the Committee on geological surveys be requested to consider the propriety of approving and furthering the memorial of the American Academy of Arts and Sciences, and other societies in behalf of a new geological survey of the State of Massachusetts.

The Association accepted the invitation to hold its next meeting at Detroit, on the second Wednesday in Aug., 1875. Adjourned to sections.

The seventh day's Session commenced on Wednesday at 10 A. M. After the election of new members, the following recommendations of the

* The Constitution, as finally adopted, is printed in the first part of this volume.

Standing Committee were submitted and afterwards adopted by the Association. That the report of the Sectional Committee of Sec. A on the list of papers to be published be accepted subject to revision of the Secretary of the Section. That the Association be notified that in consequence of the absence of all the officers of Sec. B, that Section was entirely without representation. The Standing Committee therefore is unable to take any action on the papers of that Section. That Mrs. Elizabeth Thompson, of New York, having donated to the Association at the Portland meeting the sum of one thousand dollars, be classed as a patron of the Association. That a Subcommittee, consisting of the president, vice president and three other members be appointed to whom the Standing Committee delegate its full power during the interval between this meeting and the next, and who shall be especially charged with the careful supervision of the volume of Proceedings and the exercise of careful economy in its preparation. [The chair appointed Profs. Gray, Lovering, and Morse as this committee.]

That in view of the scanty means of the Association the volume of Proceedings for the year be restricted to 450 pages in full, if practicable.

Henry Wheatland and G. F. Barker were appointed Auditors.

On motion of Prof. Lyman it was resolved that the Association look to the Chairman and Secretary of Sec. B, for the papers of that Section, and that they be sent to the Permanent Secretary. It was also resolved to notify Sec. B to hold an informal meeting after the adjournment.

The following votes of thanks were passed by the Association:—

Offered by Mr. Wheildon, to the Local Committee and citizens.

Dr. Morris, to Musical Societies.

Mr. Wilbur, to Colt Fire Arms Co.

Mr. Wilbur, to President of Trinity and Wesleyan Colleges.

Mr. Wilbur, to Mrs. Colt and Mr. Batterson.

Prof. Lyman, to Railroad Companies.

Mr. Hedrick, to invitation from Williamsport, Pa.

Dr. Elliot, to Executive and Legislative bodies of Conn.

Dr. Smith, for invitation from Detroit, Mich.

Mr. Wheildon, to Dr. Stearns of Retreat for the Insane.

Mr. Gunning, to Prof. Rice of the Local Committee.

Prof. Brewer, for invitation to Cheneyville.

Mr. Gunning, to Rev. W. L. Gage, Secretary of the Local Committee. [verbal.]

Dr. Gould, to President Le Conte. [verbal.]

Mr. Wheildon, to Public Press. [verbal.]

After brief remarks by president Le Conte the Association adjourned to meet at Detroit, on the second Wednesday in August, 1875.

The reports of Sec. A and its subsections are herewith submitted. Those of Sec. B have not been received.

A. C. Hamlin,
General Secretary.

VOTES OF THANKS.

Resolved, That the thanks of the Association be presented to His Excellency, Gov. Bagley of the State of Michigan, to His Honor Hugh Moffat, Mayor of the City of Detroit, to President J. B. Angel of the University of Michigan and to Geo. P. Andrews, Esq., President of the Detroit Scientific Association, for their several very courteous and appreciative invitations to hold our next annual meeting in the city of Detroit, which have been most cordially accepted in accordance with the recommendation of the Standing Committee, and that the President be requested to communicate this action of the Association to each of the parties named.

Resolved, That the thanks of the Association are hereby expressed to the executive and legislative authorities of the State of Connecticut for the early provision made by their authority for the use of the State House and its commodious apartments by the Association, its sections and its officers.

Resolved, That thanks be given to the janitor of the State House and his assistants for the promptitude with which they have attended to the wants of the Association and assisted its officers in the performance of their duties during the meeting.

Resolved, That our thanks be hereby expressed to His Honor Martin Powell, Mayor of the city of Williamsport, Pa., and to Hon. J. O. Parker, Chairman of the Committee of Citizens, for their very kind invitation to hold the next meeting of the Association in that city.

Resolved, That our thanks are hereby tendered to Mr. J. G. Batterson for the invitation to visit the polished granite works and sand blast of Messrs. Batterson, Campbell & Co.; also to Mrs. Colt for an invitation to visit her grounds during her absence from the city.

Resolved, That our thanks be tendered to Prof. Brocklesby, acting President of Trinity College, to President James Cummings of Wesleyan University, to E. B. Hewes, Esq., warden of Connecticut State Prison, for their respective invitations to visit those institutions.

Resolved, That our thanks be tendered to Gen. Wm. B. Franklin, Vice President of Colt's Fire Arms Manufactory for an invitation to visit that establishment, to Dr. R. J. Gatling for explanation and exhibition of the Gatling Gun, and also to Mr. Chas. B. Richards, engineer, for attention to our members on the occasion of our visit to the Manufactory.

Resolved, That our thanks are hereby expressed to the German Quartette Club of this city, the Park St. Church Quartette, and to Geo. W. Steele, organist of Pearl St. Church, for their generous services on the

occasion of our excursion down the Connecticut River, which added so much to the refined and delightful enjoyments of the day.

Resolved, That the thanks of the Association be presented to Dr. H. P. Stearns, Superintendent, and to the Board of Directors of the Retreat for the Insane, for their kind invitation to visit that admirable Institution and its grounds, and for the generous entertainment provided for the members and their ladies.

Resolved, That the thanks of the Association be presented to the Hon. H. C. Robinson, Chairman, Prof. Brocklesby and J. M. Allen, Vice Chairmen, Rev. W. L. Gage, Secretary, and the members of the Local Committee of the city of Hartford, and the town of Middleton for their thoughtful and efficient arrangements for the accommodation and entertainment of its members during its present meeting, and especially for the various festive and scientific excursions so generously and liberally provided for in the interest of social enjoyment and scientific research; appreciating all these not as personal to ourselves but as evidence of the interest of the citizens of Hartford in the advancement of science as the basis of progress and refinement.

Resolved, That the thanks of the Association are hereby tendered to the manager of the New York, New Haven and Hartford R. R.; the Boston and Albany R. R.; Connecticut Western R. R.; Connecticut Valley R. R.; Connecticut River R. R.; Boston, Hartford and Erie R. R., and to the Hartford and New York Steamer Co., for their liberal arrangements for the accommodation and conveyance of our members over their respective lines at the close of our meeting.

Resolved, That our thanks are also respectively tendered to the Connecticut, Western, Hartford, Providence and Fishkill, and the Connecticut Valley railroads, for special favors in the several excursions over their lines.

Resolved, That the thanks of the Association be tendered to Messrs. Cheney and brothers for the kind invitation to visit their works at South Manchester, an excursion which gave great pleasure to many members.

Resolved, That our thanks are hereby tendered to Prof. Rice for invitations to Tariffville, Portland and Middleton, for his guidance to objects of geological interest and for his hints and suggestions for examining the same.

Verbal Resolutions of thanks were also passed to the President, the Secretary of the Local Committee, and to the representatives of the Press.

REPORT OF THE PERMANENT SECRETARY.

The following cash account covers the period embraced between the first day of the Portland meeting August 10, 1873, and the closing of the account on August 8, 1874.

There were several extraordinary expenses incurred during the year, which will require economy the ensuing year to make good; but there are no outstanding debts except the balance of $808.94 due the Permanent Secretary on account of salary. The increase of salary from $500 to $1000 was made at the Portland meeting, as it was evident that the Secretary required an assistant in order to attend to the greatly increased business of the Association. In consequence of this action an assistant has been constantly employed at the permanent office in Salem since the receipt from my predecessor of the Dubuque volume for distribution.

The unusual size of the Portland volume, the numerous illustrations which it contained, and the extra cost of its distribution to members through the mail: the reprinting of volume one of the Proceedings, and the printing and distribution of the Report of the Committee on the Revision of the Constitution are all expenses of a character seldom occurring. On the other side it will be noticed that the receipts from the sale of volumes during the year were far above the average, while those from members were also above the majority of preceding years.

It is certainly encouraging to the financial affairs of the Association that all these extra expenses were met without calling on the small reserve fund in the hands of the Treasurer, and to feel that the small deficit will be made up in all probability from the surplus of the receipts from the present meeting over its expenses.

On May 14, 1874, the Treasurer, then on the eve of departure for Europe, reported that he held to the credit of the Association the sum of $1,589.23, which embraces the amount, with accrued interest, of the money that had been saved by the very careful and judicious management of my predecessor, who for so many years labored so faithfully for the Association in the performance of the duties placed in his charge.

In presenting this first report as the incumbent of an office where I have already had the opportunity to see many of the difficulties with which its holder must contend in the faithful and impartial management of his trust, I can but allude to the debt of gratitude which I feel confident the Association must fully realize that it owes to one, who, for fourteen meetings, in addition to the several years during which the meetings were suspended, has so successfully administered the executive duties and has done so much to bring the Association to its present high and important position.

F. W. Putnam,
Permanent Secretary.

Salem, Aug. 8, 1874.

CASH ACCOUNT OF THE PERMANENT SECRETARY.

American Association in Account with F. W. PUTNAM.

DR.		*CR.*	
To General Expenses of Portland Meeting,	$ 146 45	By Assessments for Meetings previous to the 22d,	$ 591 00
Printing, Engraving, Plates, and Binding Portland vol.,	2,471 71	" " 22d Meeting,	1,641 00
Circulars, Printing for Committee on Constitution, Printing for Forest Committee, and Stationery,	338 60	" " 23d "	282 00
Postage,	216 29	Entrance fees for 22d "	495 00
Express,	30 70	" " " 23d "	15 00
Sundry Expenses,	17 30	Volumes sold,	237 00
Rent of Office,	50 00	Premium on gold, and postage received,	51
Reprinting Vol. I,	279 67	Received from Joseph Lovering,	480 27
Permanent Secretary *on account* of Salary,	191 06		
	$3,741 78		$3,741 78

I have examined the above account and certify that the same is correctly cast and properly vouched.

SALEM, Aug. 8, 1874.

H. WHEATLAND, *Auditor.*

STOCK ACCOUNT OF THE PERMANENT SECRETARY.

At the close of the last account, September 1, 1873, there were on hand ten thousand two hundred and seventy volumes of Proceedings of the Association (including a few copies of the Transactions of the old Society). Nine hundred and twenty-two copies of the Portland volume were received from the printers in June, and two hundred and fifty copies of volume one, reprinted, in August, 1874. Since September 1, 1873, one hundred and eighty-two volumes have been sold, eighty-five have been delivered by direction of the Standing Committee to various institutions, and six hundred and seventy-four have been distributed to members.

The above is tabulated as follows:—

Volumes on hand Sept. 1, 1873	10,270
Portland volume received from printer	922
Volume I, reprinted	250
	11,442

Distributed to members	629	copies of	Portland	vol. and	45	other	vols.	= 674
" to Institutions	75	"	"	"	"	10	"	"	= 85
Sold	33	"	"	"	"	149	"	"	= 182
	737				204			941
Leaving on hand to new account								10,501

F. W. Putnam,
Permanent Secretary.

Salem, March 15, 1875.

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